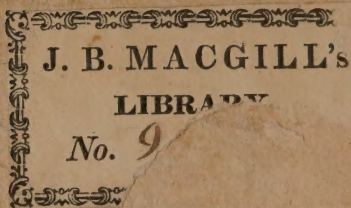




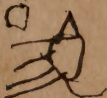
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# STRICTURES

ON THE

ELEMENTA MEDICINÆ

OF

DOCTOR BROWN.

BY

PHINEHAS HEDGES.

HUMANUM EST ERRARE.

G O S H E N :

PRINTED BY DAVID M. WESTCOTT.  
MDCXCXV.

Is. B. McGill  
jus liber.  
presented by  
Doct. Johnathan Hedges  
Newburgh  
August 6<sup>th</sup>  
1826

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349176



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TO the PHYSICIANS of the UNITED STATES.

Gentlemen,

THE following observations were committed to paper in the intervals of business; perhaps they want that connection necessary to give a clear and comprehensive view of a subject involved in so much obscurity by the author of the *Elementa Medicinæ*.

The necessity and propriety of animadverting upon *Doctor Brown's* theory, I have long thought sufficiently obvious; and I have waited with a considerable degree of impatience in expectation of some more able pen to take up the subject—But as no combatant has appeared, I think myself amply justified in entering the field of argument.

I have all along adhered to the new phraseology of the author; not so much on account of its aptness to express the different conditions of the animal œconomy, as to shew the inconsistencies and futility of his principle.

In my quotations from the *ELEMENTA*, I have in many instances abridged the verbosity (or rather expletiveness) of his style; but in no instance have I intentionally altered the sense of the paragraph. If it is necessary that I should declare and avow the motives which led to these strictures—I answer, that a forcible conviction of the dangerous tendency of extending the stimulant

mulant plan of cure, introduced and impressed with so much violence by the author, was the most urgent inducement—And whatever reception it meets with, I am conscious that I have uniformly endeavored to set forth the truth.

The number of *Doctor Brown's* disciples in this country, is to me unknown. But whether small or great, it would not be unexpected for my work to meet with many a severe criticism in their hands. I am perfectly willing it should be brought to the touchstone of reason and discussion ; and if any one discovers any material error, I shall acknowledge it.

## THE AUTHOR.

Little-Britain, Ulster County, }  
August 10th, 1795. }

DISTRICT OF NEW-YORK, fs.

**B**E it remembered, that on the eighteenth day of August, in the twentieth year of the Independence of the United States of America, Phinehas Hedges, hath deposited in this office the title of a book, the right whereof he claims as author, in the words following, to wit :

**S T R I C T U R E S**  
**ON THE**  
**E L E M E N T A M E D I C I N Æ**  
**OF**  
**D O C T O R B R O W N,**  
**BY**  
**PHINEHAS HEDGES.**  
**HUMANUM EST ERRARE.**

*In conformity to the act of the Congress of the United States, entitled, "An act for the encouragement of learning, by securing the copies of maps, charts and books, to the authors and proprietors of such copies, during the time therein mentioned."*

**ROBERT TROUP,**  
*Clerk of the District.*

# THE HISTORY OF THE

REIGN OF

EDWARD VI.

BY

JOHN STOW.

IN TWO VOLUMES.

LONDON,

PRINTED BY

J. STOW.

1674.

IN TWO VOLUMES.

LONDON,

PRINTED BY

J. STOW.

1674.





# STRICTURES

ON THE

ELEMENTA MEDICINÆ

O F

DOCTOR BROWN.



THE world has lately been favored with a production in the medical art, which if its merits were equal to the boastings of its author, we would not have occasion to lament the fatal termination of a great part of the disorders which afflict the human race. But unfortunately, this boasted production, when examined, and its principles applied to practice, leaves the professors of medicine in the same uncertainty and conjecture, which has ever pervaded that science. The author in his preface, begins with an affected and false representation of the dark, ignorant, and baneful state of medicine when guided by the theories heretofore and at present in vogue among the professors of medicine. He represents himself to have been for a number of years in a wilderness of darkness ;

to have followed with implicit faith the wild conceits of former theorists—But to his utter astonishment, out of this great and impenetrable darkness, he was brought into marvellous light. It was at that important æra that the genuine principles of medicine were discovered ; for what was called medicine before, was a false conception, and always in practice ended in abortion.

This discovery was not the result of reasoning from analogy ; for every principle heretofore set up to unravel the mysteries of the animal œconomy were illusory and equivocal. The author undoubtedly had a design in this representation of his discovery. Unwilling that the world should believe his system, resembled the system of Cullen—he took uncommon hypocritical pains to amuse the public with the pretended novelty of its principles. It requires but a superficial acquaintance with the works of Cullen, to discover that the principle laid hold of by Doctor Brown, and carried through his work is nothing, more or less, than the principle of Cullen, used in explaining the different conditions of the brain in *mania* and *melancholia*, and that principle distorted and mutilated by his transforming power, into what he confidently asserts to be a new principle in medicine, and sufficient to explain all the phænomena of health and disease. By this discovery, he has changed physical uncertainties into mathematical certainties.

He has pointed out the salutary and hurtful  
powers

powers of the human body ; he has invented a scale ; a criterion by which you can discover the former and avoid the latter, and by which you can always ascertain whether the excitement is raised or fallen below the point of health—In short, by this effulgent discovery, medicine is reduced to such a degree of simplicity, that it requires very little knowledge except a ready acquaintance with addition and subtraction. For if the excitement is two degrees above the healthy point, subtract two ; and if two below, add two, and health will infallibly succeed. Having made these general observations on the *Elementa Medicinæ*, we propose to make some strictures on the work, in the course pursued by the author. In the course of these observations I may occasionally touch upon the theory of Doctor Cullen, in order more forcibly to illustrate my ideas—and if it appears that I am a disciple of Cullen, it is from a thorough conviction of the truth and importance of his doctrine. I by no means wish to be a blind supporter of the writings of Cullen, but I think their truth, ingenuity and excellence, are sufficiently obvious.

In the beginning of his work our author defines medicine to be preserving the good, and curing the bad health of animals. Is this definition logical ? Is not health a point the least deviation from which constitutes disease ? I would not wish to appear captious ; but an author who challenges all the world, should be severely criticised.

Paragraph VI. *General disorders are always, and local never preceded by predisposition.* I object to the first part of this proposition, for this reason, that persons who from all appearance of sense, life and motion, are endowed with the same temperament of body, are not all affected with disorders which are supposed to be epidemic in epidemic seasons ; and further, that persons whose temperament appears most repugnant to the operation of causes which produce epidemics are sometimes affected as well as those of a temperament more favorable to the operation of epidemic causes, and that in the most violent manner. But if predisposition always preceded general disorders, we should suppose that predisposition together with the operation of general causes affecting all men alike would uniformly in persons predisposed produce the disorder. But this is contrary to experience, and medical facts are so various and diversified that it is difficult to establish this opinion on any sure foundation. Local disorders, as boils and eruptions ; as certainly depend on the diathesis as general disorders and according to our author the diathesis is both predisposition and the disorder. It is a very rare occurrence for boils, which are the effect of an inflammatory disposition, to affect persons of an emaciated spare habit of body. And as they are the constant attendants (whenever they happen) of an inflammatory diathesis, we suppose they are accompanied with pre-disposition the diathesis.



Our author in paragraph LXXIII, defines predisposition a middle state between health and disease ; but this definition conveys no adequate idea ; for prior to our conceiving what he means by the term pre-disposition, we must understand what is meant by health and disease. He has confined himself to the literal etymology of the word which signifies to be placed, or go before a thing which succeeds it ; but this definition conveys no idea of the state of the system. In a succeeding paragraph, he says pre-disposition arises from the same exciting causes, acting upon the same excitability, and in fact is part of the disease. Predisposition I conceive to be a peculiar disposition or organization of the muscular fibres of the body, either laid in the original stamina, or induced by disease or habit. The muscular fibres of the living system, are more or less tense and elastic, and are destined to act with more or less vigor—but as there is a certain point of tenseness and elasticity of muscular fibre common to the whole, or almost all the human race, and as yet there may be considerable variations in different persons, without impairing health, physicians have denominated this deviation from the common standard, pre-disposition. It will readily be seen that pre-disposition may be coeval with the first rudiments of the fœtus. It is very apparent that some children have more lax and flaccid muscular fibres than others ; whether owing to the rudiments of the fœtus or circumstances affecting its nourishment in utero, we will not affirm. A person of a lax fibre, is generally

nerally supposed to be more disposed to disorders originating from debility, than a person of a tense elastic fibre. The lax fibre however, is constitutional and not owing to the same exciting powers which produced the disorder and constitutes no part of it. The same quantity or energy of exciting power that will produce a disorder in one person, will not in another, which is the reason why we say the former is pre-disposed to the disorder, and the latter not. Our idea of pre-disposition is synonymous with our author's, more or less abundant excitability of the animal œconomy—He says that there is a certain portion of excitability assigned to every living system, and it will readily be seen that where there is a great portion of it assigned, that body will be affected both with the salutary and hurtful powers more than a person possessing less of it ;—from whence it clearly follows that a person possessing a great degree of excitability is pre-disposed to disorder of the inflammatory class, and a person possessing less, to disorders originating from direct debility.

Paragraph XXI. Our author has introduced a new theory of the passions. In the explanation of this subject, he affirms that the sedative passions as fear and grief arise from the same causes, namely exciting powers—Although grief and fear are produced by powers which may be called stimulant, or exciting in comparison of the entire absence of those powers, yet in comparison of the natural excitement of the mind they produce

duce a sedative effect, and may therefore according to the author himself be said to be produced by sedative causes. For in a note upon paragraph XX. he introduces what he calls a new mode of reasoning into medicine, that is, from an identity of effect, he infers an identity of cause. In the passions of grief and joy, two effects diametrically opposite, both physical and moral are produced; from whence we infer a diversity instead of identity of cause. Our mode of reasoning on this subject may aptly enough be compared to the different national denominations of money—here we establish an unit, and our calculations always have a direct reference and comparison with this established unit. The natural excitement of the mind may be called the unit, from which we reckon the various degrees of health and disease, and the effect produced on one side of this unit we attribute to the operation of different causes from that on the other. A passion which lowers the excitement and vivacity of the mind, we call a sedative passion, and that for the same reason that we say a shilling is less than a dollar, and more than a penny. The author's reasoning amounts to as great an absurdity as to say that as all the causes which beget and support vegetable life are stimulant, therefore all the variety of vegetables are one and the same production. The discrimination of visible and mental objects is of the utmost consequence in the communication of knowledge.

In a note upon paragraph XLVII, we are told

told that indirect debility commences at 70° of his scale and continues to 80°, the death point. From what data he infers that indirect debility, begins at the precise point of 70° he has not informed us, neither does it appear to me that this boasted scale by which we are to gauge the vital principle, can be of any utility or advantage in the administration of medicine—some of the disorders placed above 70°, appear to me to depend on too much phlogistic diathesis, or excitement; whereas if they were the effect of indirect debility a mode of cure opposite to that generally followed, should be adopted in those disorders. It would appear rational to conclude, that whenever indirect debility begins, that the excitement should be lessened. Our author however, supposes that the excitement is still increasing from 70° to 80°, notwithstanding the approach and existence of indirect debility, which is as absurd as to affirm that 70° minus 10 amounts to 80°. The continued repetition and operation of exciting powers, often induce indirect debility, when their aggregate operation has never at any one time been sufficient to raise the excitement to 70°. In the apoplexy we generally suppose a rigid tense fibre to exist, and the symptoms all serve to evince that this disorder depends on too great an excitement or inflammatory diathesis, and the mode of cure in these cases is conformable to this generally received opinion. We imagine the author to have denominated an effect of disorders, their cause. The idea of indirect debility pre-supposes that the exciting powers have



have been applied to a very considerable degree, and as whenever the excitement is raised above  $40^{\circ}$  and keeps within the range of  $70^{\circ}$ , a disorder depending on too great excitement must exist in some of the intermediate points, it clearly and irresistibly follows, that a sthenic disorder must precede an anti-sthenic one depending on indirect debility. Disorders of indirect debility are most generally the consequences of continued and habitual courses of gluttony and ebriety.

The operation of contagion and marsh effluvia, I presume to be of a sedative nature, or they possess a power of stimulating less than is necessary to support the healthy state of excitement. A person confined in a jail or prison-ship, never experiences increasing vigor prior to an attack of a jail fever. Whereas if the matter of contagion was of a stimulating nature a gradual increase of vigor would announce their stimulant operation. I will not contend with Doctor Brown, that these noxious agents stimulate none at all; it is sufficient reason for calling them sedative if they do not stimulate in a degree necessary to maintain the healthy state of excitement. It greatly strengthens this opinion that contagious matter generally exists in warm climates, warm seasons and confined places, when the elasticity and density of the air is greatly diminished.

From facts so conformable to experience we conclude, that diseases of indirect debility are not so numerous as Doctor Brown imagines, and that

that they may happen when the exciting powers have not raised the excitement to  $70^{\circ}$ , and that from their habitual repetition. It may be proper in this place to enquire what useful purpose the scale of our author can serve in the science of physic. It may assist in explaining and illustrating the principles of his theory ; but as its graduation is arbitrary, any other would have answered his purpose equally as well. Does it possess advantages superior to the arrangement of nosologists, into classes which comprehend a very general association of diseases ; into orders which are a subdivision of classes, and into genera and species which are more minute subdivisions. In the first, a very general character of disorders is given ; a few of their symptoms which are common to the whole of the class are noticed ; in the orders a more particular character is given, and in the genera and species a very minute detail and description of all the symptoms. By this arrangement, the student's attention is confined to one precise object. But in the general abstract manner in which the author has treated this subject, a student could never acquire precise ideas of the variety of complaints to which the human race are liable. A competent, adequate knowledge of the vegetable and animal kingdoms could not be obtained unless botanists and natural historians gave a minute description of any production under investigation, shewing wherein it differed from others of the vegetable tribe—Nosologists may have made more distinctions than have any real

real or useful foundation, but our author in his furious rage for novelty has levelled the arrangements of ages. I submit it to the candor and good sense of the faculty to declare whether this innovation is useful or just—diseases in their different stages are attended with different degrees of excitement, the principle of life is continually wavering, and he that is the least experienced must have observed inflammatory disorders exhibit strong marks of debility.

Paragraph XLVIII. *The seat of excitability is medullary, nervous matter and muscular solid.* But how confined or universally diffused this medullary matter is, or in what manner excitability, or excitement, is produced, the author is entirely silent. It is the professed object and design of theory to explain in a rational, consistent manner every phenomena of the animal œconomy, both in the healthy and diseased state; and we judge of the truth and utility of theory from its perfectly answering this end. It is a desideratum not yet ascertained. The essay of Doctor Brown has not enlarged the sphere of theoretical principles. Theorists have heretofore examined the fluids and solids in search of the true principles of medical science: they have by dint of experiment, discovered some of the properties both of solids & fluids, and as far as those discoveries went, applied them to the elucidation of the phenomena of the living system. But Doct. Brown, instead of exploring the animal œconomy, has set up an abstract principle.

He has neglected experiment, the clue of modern philosophy ; he has incarcerated his senses and closed up every avenue of knowledge. The term excitability, conveys no definite idea of the state of the system either in a healthy or diseased state. He does not explain the state of the solids and fluids in health, nor the change they undergo in disease. Where is the seat of medullary nervous matter? Is it in the adipose membrane? Is it universally diffused over every fibre—and is it generated in the brain—and what is the process by which food is converted into this vital substance? From what appearances do we judge that medullary nervous matter is abundant or deficient, or in proper degree? Is excitability inherent in medullary matter? Does it depend on a particular contexture of the primordial particles of medullary matter? Or does excitability, like the electric fluid, remain in a dormant state until the exciting powers, like excitation, arouse it into action?

Answers to these queries are essentially necessary to the illustration and completion of his theory. It being the foundation of his system, together with the hitherto inexplicable nature of the subject, I should have expected would induced the author to have treated the subject in a more diffusive and scientific manner. We presume that the notion of medullary matter's being the seat of excitability, is not a part of that gleam of light that beamed upon his mind at the time of his discovery, but conclude the doctrine

to have been taken from Kirkland and others who slightly touched upon this subject. Excitability in its seat, causes and effects, are but indifferently explained; and, in fact, the author is so candid in the beginning of his work, as to say the uncertain nature of the subject and the poverty of language rendered it difficult to speak very intelligibly on the subject. I ardently wish the author had pointed out the laws which regulate the excitability and excitement in a more perspicuous manner. But we are informed of this *only*, that in proportion as the excitement increases, the excitability decreases. This was known to every one the least acquainted with the animal œconomy before the luminous discovery of Doctor Brown: For as there is a certain degree of activity or energy capable of being exerted, when that energy is raised, there must be less to raise, unless the quantity was infinite.

Paragraph L. The author endeavors to inspire a belief that the notion of medicines operating on a particular part, in preference to every other part of the system, has been exploded in consequence of his discovery. It is well known, however, that it has been an obsolete, exploded opinion for near half a century. But the author must represent the system of medicine different from what it really is, before he can convince the world that he has effected a revolution in the principle of medicine.

Paragraph LXIII. *Medicines are not to be directed*



*rected to the solids or fluids, but to the diminution or encrease of excitement.*

I confess this to be a new doctrine in medicine, and I believe as false or imaginary as new. It is beyond all conception to conceive how a medicine can effect any change in the state of excitement, without operating on the solid parts, the seat of excitement. The opinion of medicines operating on the fluids, has been satisfactorily refuted by Doctor Cullen. And our author, with an insatiate desire of novelty, has excluded both solids and fluids from his pathology of disorders. In what part of the body does he expect medicines to operate? If they do not come into contact with some part we can expect no success from their exhibition; and if they do come in contact with any sentient part, an operation on the solids is the infallible consequence. Medicines do not operate upon the living principle immediately, but mediately.

To direct medicine to the excitement only, appears like administering medicines to spiritual beings who possess neither parts nor space, in consequence of which the operation of medicines would be entirely incomprehensible.

Although we generally suppose debility to be the effect of causes operating upon the vital principle, we commonly connect an idea of laxity in the simple solids, and our medicines are directed with a view of altering the contractility and tone  
of

of the fibres of the body. I would not be understood to mean the simple solids strictly so called, but every fibre, whether distinguished by the name of simple or living solid.

The force of cohesion in the component particles of a fibre, we suppose to be lessened in debility. This opinion our author confirms when he says that the force with which muscular fibres resist stretching, is their density. And as he affirms that the force with which a dead and living fibre resists stretching, is greatly different, and as every encrease of debility is an approach to the state of death, therefore the idea that the force of cohesion, or density in the component particles of a fibre is lessened in debility, is rendered probable from his own concessions.

There is a certain cohesion of every particle of the body on which the most perfect health depends, and on which healthy excitement depends; it therefore follows that the state of the solids is an object of no small consequence in the treatment of disorders.

To exhibit medicines with an abstract view of altering the excitement, is a scheme too visionary for a serious mind. Medicines act on the living system, not as matter simply, not as excitement simply, but on the two whose union we imperfectly comprehend. They act and re-act upon each other; they produce phænomena which

which the utmost industry and research have never been able fully to investigate and develope.

Paragraph LXV. *The notion of health and disease being different states, is disproved by the operation of the powers which produce them, and those that remove them being one and the same.*

The powers which support life and remove disease are the same, as they agree in the common notion of being matter, but in their effects on the diseased and healthy state, they are entirely and specifically different. Food and medicine have a very different operation on the animal œconomy; and although the author may say their difference of operation consists in degree only, there is this, however, to be observed, that the former is overcome and assimilated to the body, and the latter not. All farinaceous, saccharine and oily substances, are convertible by the powers of digestion, into succum et sanguinem; but a person may eat opium, mercury, and all the chemical medicines, and they will never be converted into those juices which are suited to repair the daily waste of the body. Medicines stimulate directly, and food indirectly. A full meal, when in contact with the stomach, will encrease the velocity of the pulse in a small degree; but food is not taken barely with an idea of stimulating the muscular fibres of the stomach. Its stimulating and exciting the stomach into contractions promotes its digestion; but it supports life in a different manner. Withhold every nutritious

tritious substance, and exhibit a quantity of any stimulant power equal to a full meal, and life will be of short duration.

The principal intention of food is to be converted into blood, and stimulate the sanguiferous system. Medicines operate on the nervous system. Add to this, I assert the physical condition of the body in health and disease are entirely different. Are the freezing and boiling points of water the same? Does not health consist in a proper degree of energy of all the functions? and does not disease consist in a perturbed, diminished or encreased energy of the functions? To render the absurdity more palpable, —has he not defined pre-disposition to be a middle state between health and disease? Whereas if health and disease were the same, there could be no middle state; for the very idea of a middle state, supposes two extreme opposite states.

Paragraph LXXXIX. *The same debilitating remedies that remove any one sthenic disease, remove all of that class, and the same stimulant remedies that remove any one asthenic disease remove all the rest.*

To prove this assertion the author must either exclude a number of disorders that he has arranged under the two different classes, or deny facts which every days experience incontestibly proves. The rickets, the scrophula, and many other diseases which he arranges in his asthenic  
form.

form, too often elude the force and power of the most sovereign tonic and stimulant remedies; while the intermittent fever, a disorder of the same class, gives way with great ease to the efficacy of bark and wine. This fact is a full refutation of the assertion:

I believe that however greatly desirable simplicity and uniformity may be in the cure of disorders, we often fail by giving way to reasonings of this kind. I acknowledge, however, that reasoning from analogy is the only substitute for actual experience. But I wish to caution the faculty against an implicit belief and confidence in this fallible criterion of truth. The author, in his division of disorders into two classes, and his indications of cure in consequence of the division I fear is too general, and if indiscriminately followed would be productive of serious and alarming consequences to the unhappy sufferers.

There are a variety of circumstances to be considered in the cure of disorders. A physician should give a wider range to his contemplations than barely the state of excitement. The state of perspiration, the alvine discharge, the quantity of urine, all ought to arrest his attention. And although Doctor Brown may assert that a restoration of healthy excitement would establish the natural vigor of these functions, I am disposed to believe, that a medicine possessing a less degree of stimulant operation, and at the same time exciting perspiration, would have a more  
salutary



salutary operation in many cases than a medicine not disposed to increase the cutaneous discharge, and yet possessing a greater stimulant power. The body was considered as a whole, long before the publication of Doctor Brown's theory; but that whole consists of separate parts, all connected and combined together by a general principle. The different parts being differently organised, and those distinct organs having distinct offices assigned, will no doubt modify the operation of medicines, notwithstanding their general operation on the excitability. It is an undeniable fact, that there are medicines which increase perspiration and the urinary discharge, whose operation can in no manner be accounted for on the author's general principles. Would it not have been in more perfect consistence with the author's theory, to have enjoined abstinence from all remedies in sthenic diathesis. For as all the powers in nature are of a stimulant operation, although you administer a medicine possessing 20° of stimulant operation in a diathesis of 60°; yet as it stimulates some, it must serve to keep up the diathesis longer. Physicians have generally advised abstemiousness in diet and plentiful dilution in inflammatory disorders on this principle. In a system which pretends to demonstration and mathematical exactness and certainty, it would appear proper to state the length of time necessary for a medicine to raise deficient, or diminish increased excitement to the point of health. This would have been the touch-stone of the theory under discussion. From

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the author's attention to exhibit a medicine of exactly as much less exciting power than  $40^{\circ}$ , as the excitement is raised above  $40^{\circ}$  it would appear that he is fearful of debility in the employment of less exciting power; this indirectly conveys an idea that the operation of medicines is instantaneous—for if their operation was not so, there would be no hazard in the administration of a medicine possessing less stimulant power than  $20^{\circ}$ , when the excitement is  $60^{\circ}$ . Experience however, proves that medicines are slow in their operation, and that in the morbid state a continual repetition of exciting power, is necessary to maintain the system even in statu quo. Let the excitement be reduced to  $20^{\circ}$  in a disease of debility, it would then appear according to the principles of our author that by adding to  $40^{\circ}$  (the stimulant power necessary to support health)  $20^{\circ}$ , that the excitement would be restored to the healthy point. But if we concentrate all the stimulant and nutritious power of a full meal, and add half its quantity, the disease will maintain its obstinacy. The hurtful powers which produce disease, exist without the body, they operate on the surface, and medicine on the stomach; they are invisible, untagible and escape examination. It is not only necessary to exhibit a quantity of stimulus as large as is necessary to support health and take off deficient excitement; but it is also necessary to exhibit a power whether stimulant or sedative, sufficient to counteract those hurtful powers—for the cause is supposed still to operate during the exhibition of

of medicines, in consequence of which we have both cause and effect to combat. If the cause was removed, the power of nature would be sufficient to restore the healthy state. These observations apply to both the states of encreased and deficient excitement.

It may be necessary to remind the author that he has asserted, that every medicine operates as a stimulant. Although venesection would not come within the definition of the word medicine, it is a means employed to restore the healthy state, and one might in effect affirm it to be a positive sedative power. It operates by an abstraction of stimulant power, without any stimulant operation, except the mere solution of continuity. Its effect in sthenic diathesis is greater than any other evacuation, and proves that an abstraction of all exciting power, agreeably to the true principles of this system, to be proper and adviseable. Although the stimulant operation of evacuants is more than compensated by the discharge, they are hardly admissible on the sound principles of this theory.

There is a class of medicines called refrigerants, whose operation in inflammatory disorders is to abate excess of action. Extreme action of muscular fibres or of the arterial system, always generates heat, and as refrigerants, even when exhibited in so small a quantity as to produce no evacuation, appear to diminish excessive action; they therefore have a positive sedative effect.

When

When dissolved in water they increase the coldness of that element, and when administered in disease, their operation I suppose nearly similar.

When given in health they counteract the stimulant effect of food and the other non-naturals, and diminish the temperature of the system below the healthy point, which is what all physicians have meant by sedative powers. The author's idea of a sedative, is such a power as would instantaneously destroy the vital principle. How far some poisons might come within this description of a sedative power, every one may judge who has made any observations on their sudden deliterious consequences. Do they cause dissolution in consequence of a positive sedative power? Or do they induce indirect debility? If the latter, their visible operation does not manifest it: For as soon as any operation is perceivable, all the symptoms shew diminution of action; whereas, if a stimulant operation intervened between their exhibition and indirect debility, some symptom should manifest it. If their stimulant operation is so sudden as to instantly raise the excitement within the range of indirect debility, from what data can we infer a stimulant operation. We have no knowledge of medicines but from their evident effects on the body. Most gummy and resinous substances have a stimulant effect, and by analogy we might infer, that poisons possessing a gummy or resinous part, would have a stimulant operation. But can we prove that they do not contain other principles  
which

which not only counteract the stimulant operation of the gummy and resinous parts, but possess a power so strongly sedative as to induce immediate dissolution. Even chemical analysis of the component and elementary parts of a substance would not be an infallible test of their operation on the animal œconomy. Some of the parts are dissipated in the process of separation, and often when the separate elementary principles are exhibited singly, they shew different effects than when exhibited in combination. If our medicines were directed to the excitement only, we need make no enquiries about the exercise of the different functions. The pulse would be a certain index of the excitement, and all the business of a physician would be to encrease or lessen it as occasion required. Costiveness, strangury, and deficient perspiration, would no longer engage our attention,

The vital principle is the bond which connects all the functions together; but they are so independent of each other, that very often, while one is entirely vigorous and unimpaired, another is impeded in its exercise. It is also an established truth, that in many disorders of debility, where costiveness and many other functionary complaints exist, that they are not to be removed in any other manner than by the administration of a medicine suited to relieve that particular symptom. And although in its operation a cathartic may appear to increase the debility, it is attended with the most salutary effects, and is



a necessary prelude to the successful administration of stimulants. Suppose an infarction of the lungs to happen in a state of evident debility, would a medicine of high or moderate stimulant operation, unaccompanied with the power of promoting perspiration or expectoration, remove the infarction as certainly as a medicine of the same stimulant operation possessed of that power? I fancy the experience of most of the faculty will answer in the negative. And if it is true also, that by the exhibition of refrigerants and other medicines in sthenic diathesis, the disease will give way sooner than to withdraw all nourishment and medicine, it then indubitably follows that these medicines possess a positive sedative power.

Paragraph XCV. *In the cure of diseases we must always stimulate or debilitate, never lay by, nor trust to the supposed powers of nature which have no real existence.*

That the human body contains within itself a principle of preservation and renovation, is proven from facts so numerous and notorious that they cannot escape the attention of any person of observation. This opinion has been coeval with the first rudiments of medicine—& the principle is coeval with the embryotic state of man. Its belief has been universal, and although the universality of its belief is not an absolute proof of its truth, yet the numerous facts which corroborate and strengthen it, make it a truth as undeniable as any in the science of medicine.

This principle is diffused throughout all nature. Even inanimate matter seems to possess a principle of this kind. The attraction of cohesion existing in all matter, appears to be the same principle as the *vis medicatrix* in man.

What is it but this principle that causes the eyelid instantaneously to cover the eye whenever the least danger approaches? What, except this principle, is it that causes the appetite of hunger to return at those periods when the exigencies of the constitution require food and nourishment? We are not left to the slow dilatory decisions of reason. In our appetites we are governed by instinct, and instinct is nature. The busy avocations in which men are employed, would lead to a procrastination of the necessities of life, were they not impelled by the painful feelings of appetite. Is it not the efforts of nature that incarnates a solution of continuity? If our author answers food, I reply that a particular organization of the digestive and assimilating organs, and a particular contexture of the vessels of the part, is the *causa sine qua non* of this new growth of flesh. The efforts of nature often restore persons to health, without the exhibition of any medicine, and sometimes in spite of medicine. It cannot be imputed to the operation of food; for it is well known that diseased people generally have a disrelish and disgust to every species of nourishment. Admitting that they ate what would support health, in a healthy state, it would not be equivalent to repair the waste of excitement

excitement from disease. It may be answered that the exciting hurtful powers are removed. Pollutions of the atmosphere, from the putrefaction of animal and vegetable substances rising in the vaporous form, are assigned very justly as a powerful cause in the production of intermittent and remittent fevers; and persons recover very often from fevers imputable to this cause without any medical aid, while others are daily attacked with the same complaint. This fact will not justify the inference, that the cause is removed. To what shall we ascribe this fact, except to that principle in the constitution of man, whereby when any noxious powers are raised against the body, the *vis medicatrix* is excited to obviate the lethal tendency of those powers. The diurnal revolution of fevers is a full confirmation of the existence of the *vis medicatrix*. We shall not in this place discuss the theory of fevers; but if such a principle did not exist, would the cold, hot, and sweating stages of fever, follow one another in regular measured order? Would a final solution take place without the administration of medicine? A state of debility or encreased excitement on the principles of this theory, could never be recovered from. The system, without medical aid would be condemned to perpetual and encreasing disorder until relieved by dissolution.

The constant tendency to decline after the body arrives at its acme, the numberless noxious agents that exist without, would expose us to numerous infirmities, we would be constantly  
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In jeopardy of death, we would have continual need of the physician, and the lot of man would be miserable indeed.

Why does the stomach reject food when it is of a nature not calculated to nourish the body? Nausea and vomiting are the first symptoms of the operation of poisons. The same principle is perceptible in the brutal and vegetable creations. Animals often recover from diseases without any assistance, and trees often decay in part without extinguishing the principle of vegetable life. Whereas, if no inherent power existed within to repel the hurtful agents, no reason can be assigned why the decay does not spread to the trunk as well as the branches. Why does the sensitive plant shrink from the touch?

The Doctrine of critical days so long observed and acknowledged by the faculty, is a confirmation of the existence of the vis medicatrix.

In numerous fevers and pleurifies that I have seen, I have generally observed a solution take place on an odd day, and very often by a considerable increase and evacuation of perspiration, urine and the alvine discharge, which could not be imputed to the operation of medicines. Patients, when every medicine had failed, when they have been consigned by the physician to the tyrant death, have suddenly recovered; an entire solution of the fever, and every symptom characteristic of the disorder has disappeared,  
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without any remaining symptom, except that of debility. If life was a forced matter, if the recovery was to be imputed to the force of medicine, a more gradual diminution of the disorder would appear most natural. Facts so notorious cannot be overcome by the violent asseverations of Doctor Brown. The same principle seems to pervade the moral world. The affairs of states and kingdoms, as it were by an invincible necessity, tend to a certain point, after arriving at which, by some invisible cause, take a contrary direction. The disorder of mania often proceeds from bad to worse, until it arrives at a certain point, from which patients recover without assistance in a time much less than the time of its approach.

I am aware that apparently strong objections may be raised against this principle. It might be asked why the efforts of nature were not more likely to remove a disorder in the beginning, when nature was strong and the disorder weak. I acknowledge it an unanswerable objection. But how numerous<sup>1910</sup> and unaccountable are the phenomena of nature.<sup>12</sup>

The same objection lies against the theory of the comets, either on the principle of Sir Isaac Newton, or the more modern one of electricity. A comet is certainly more strongly attracted by the attracting body when in its perihelion, than in its aphelion; and yet in the former it is repelled, and in the latter attracted. When the influence



influence of the attracting body would appear the least, then the principle is exerted. This makes it more eminently an effort of nature. The *why* and the *wherefore* are often inscrutable : But the fact is what we are to enquire after. The Stahlian notion of two counteracting principles, divesting the *vis medicatrix* of its rationality is not so chimerical an opinion. The destroying principle is the natural tendency of the body to decay, together with the external exciting hurtful powers ; and the *vis medicatrix*, of Stahl divested of its rationality, is no other than the *vis medicatrix* of Dr. Cullen. On what the *vis medicatrix* depends, nor yet all the modes of its action, I shall not pretend to discuss. I will observe, however, that the reaction of fevers, is undoubtedly to be imputed to the *vis medicatrix*, and as the spasmodic or cold stage, seems necessary to produce the reaction, I therefore impute both the stages of fever to the efforts of the *vis medicatrix*. Contraction is a natural function of muscular fibre. It is by this mode of action, that nature operates to expel the morbid matter of contagions and poisons ; and the object of the physician is only to moderate or encrease the contraction of muscular fibre, as the exigencies of nature require. In the variola, why does it happen, that at a particular period after the approach of the symptoms, the contagion is cast upon the surface ? And why is it, that the efforts of nature expel the virus of the venereal disease without any assistance ?

The body is not entirely passive under the operation

operation of hurtful powers. Were it so, we would fall an easy prey to their operation. Doctor Cullen, in his theory of the gout, observes that a balance seems to exist between the external and internal parts. The resistance of muscular fibres on the surface to the expansive power of the heart, depends on their inherent power of contraction, assisted by the pressure of the atmosphere.

Sir Clifton Wintringham has observed, that the proportional density of the arteries to the veins, encreases with the advance of age; and it may be noted that in the death of old age, that the density and resistance of the arteries to the expansive power of the heart, is such as to overcome the latter. Although in ordinary health these two opposite powers are exactly balanced, nature has so framed the human body, that whenever the common resistance to the expansive power of the heart is increased, it indirectly excites a more than proportional energy of the heart and large arteries to overcome the resistance.

Where this extraordinary energy of the heart exists, whether it resides inherent in the muscular fibres of the heart, whether there is a continual reservoir of influence from which the heart is supplied, we will not affirm; but we observe that the vigor of muscular contraction is in proportion to its contiguity or remoteness from the heart, the central point of action of the sanguiferous

sanguiferous system. This is evident from the languor of circulation in the extreme vessels.

The author of man may have for his preservation, bestowed on the muscular fibres of the heart and large arteries, a more vigorous contractility, or have made them dependent on an unceasing influence from the brain, the centre of the nervous system, thereby to counteract the hurtful agents that surround the human race. We may observe the connexions and dependencies of the animal œconomy, but perfectly to unravel all its mysteries, ought not perhaps to be expected.

The human body differs from all machines ever invented by man. It contains within itself what has been an object of curious inquiry and diligent research by the most learned of every age and nation, "a perpetual motion." It is actuated by a vital principle, and the phenomena that it presents to the curious observer, cannot be compared to the movements of any inanimate automaton. But such a host of facts conspire to support the existence of the *vis natra*, that a man must close his eyes upon the volume of nature; he must be deaf to the suggestion of reason and experience to deny its truth. We do not contend that the *vis medicatrix* acts uniformly, nor yet always successfully. This would be to make man immortal. But in perfect conformity and consistency with every animate and inanimate existence, man contains within himself the seeds of death and life.

Paragraph

Paragraph XCVII. *The sedative power of cold, is as serviceable in the measles as in the small-pox.*

The good effects of cold in the small-pox we presume depends upon a peculiar operation on the skin. In the commencement of the eruptive fever, the application of heat is dangerous, on account of its debilitating the surface and thereby giving occasion to a large eruption on which the danger of both measles and small-pox may chiefly depend. The operation of cold appears to be partial.

The effect of vigorous contractions and efforts of the muscular fibres is to generate heat, and therefore cold as a refrigerant, counteracts the hurtful and debilitating effects of heat; but in no manner does cold appear to diminish the energy of excitement, for if it debilitated the surface we would expect a large eruption. The effects of cold in different conditions of the body may be different. Cold and heat alternately succeeding each other, in most cases increase the vigor of the healthy state: can we therefore expect, that cold increases the excitement when diminished, that it will diminish it when increased. If so, it must be from an entire different operation in the one case from that of the other. Cold in our climate, when not too long continued, instead of debilitating, always invigorates. A sedative operation and an increase of vigor, are incompatible. The good effect of cold then in the variola and measles, is from refrigeration, not passive sedative operation. In

In a succeeding paragraph, our author imputes the good effects of cold in the small-pox, to its relaxing the system, but no phenomena of the operation of cold justify the opinion, except that in extreme degree, it occasions gangrene. That it occasions constriction of the skin, in some measure similar to inanimate matter, is proven from the shrinking of the external parts, and the paleness or lividness that accompanies its application. I can by no means admit that cold relaxes the pores to give an easy free egress to the variolus matter, and thereby lighten the disease. It appears more probable that it constricts the surface and prevents the lodgment of the variolus particles under the skin. For where the resistance to the influx of the blood into the cutaneous vessels is taken off, the blood is urged forward with great impetuosity and a crowded eruption appears.

The constriction or refrigeration that cold produces on the surface, detains the variolus matter within the vessels until by their repeated action it is rendered inert. It is very improbable that cold or heat would so effectually relax the pores of the skin as to evacuate every particle of the variolus infection. Who would think of evacuating the venereal virus by an increase of the urinary or perspiratory discharge? In what manner mercury acts to destroy the pernicious effects of the virus, whether as an antidote, whether by entering into combination with the virus and converting it into a *tertium quid*, or by stimulating



ulating the vessels to subdue its nature, remains yet a subject of conjecture. But it appears plain from reasoning, that if contagious matter is generally diffused in the mass of blood, or only confined to the serosity, that as an evacuation can neither discharge the whole mass of blood with safety to life, nor yet the whole of the serosity, the supposition of the contagion being evacuated through any or all of the emunctories is frivolous. That cold therefore operates by giving vent to perspiration, and consequently the variolus infection in the small-pox appears equally frivolous. That cold is equally serviceable in the measles as the small pox, I shall no further animadvert than barely to observe, that if the catarrhal symptoms in the measles originate from the diathesis—it would appear rational to conclude that the removal of the diathesis would be followed with the removal of the catarrhal symptoms. I will take occasion to observe, that the alexapharmic mode of cure adopted by Doctor Sydenham, in the measles was exploded long before the publication of Doctor Brown's theory.

Paragraph CXII. *The stimulus of heat, in a moderate degree, produces its effect in due proportion, in a degree above that, the excess of its action is such as to produce more or less of sthenic diathesis.*

I cannot suppose a degree of heat above moderate, can have excess of action. The author does not inform us in what manner he graduates temperature ;

temperature ; but if we measure heat and cold by the same scale with which he measures disease, I could not believe that a few degrees of excitement above the point of health, would be followed with great excess of action. The natural temperature of the body is 98 degrees of Fahrenheit's scale, and the temperature of the air and other bodies that surround us are 62° of the same scale.\*

Doctor Cullen thinks every degree of temperature below 62° has the effect of diminishing the temperature of the body, and is consequently hurtful—any substance on the contrary, possessing a temperature above 62° will raise the temperature ; from this point of temperature then we are to trace its effects on the body. The constant and continued effects of heat or cold on the body we know very little about, for neither heat or cold are so constant in our climate, as to produce effects uncombined with other causes. I would observe however, that the constant application of heat to such a degree as to occasion an increase of perspiration above the imperceptible vapor that constantly issues from the body, has a relaxing effect. This opinion is rendered probable from the leanness and emaciation that most people are subject to in warm weather. There is as Doctor Cullen observes, a certain degree of heat necessary to support the vital principle, and we will establish that temperature to be composed of the generating power of heat within the body,

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and

\*Vide Cullen's first lines, page 88.

and  $62^{\circ}$  the temperature of the bodies which surround us keeping the temperature of the body at  $98^{\circ}$ . We will admit for a moment, that heat augments the temperature of the body. We however deny, that it increases the tone and elasticity of muscular fibre—every appearance of the operation of heat, manifests a diametrically opposite effect. The effect of heat on the body by no means corresponds with the idea of increased excitement or phlogistic diathesis. An increased velocity of the pulse, an increase of perspiration, and a lassitude and weariness which accompany the application of heat, do not evidence an increase of excitement. In the exhibition of stimulant medicines, the momentum of the pulse is increased, vigor and activity in all the limbs and motions phenomena which we generally impute to an increase of excitement. Appearances so repugnant to each other cannot be imputed to identity of cause. By what law of the animal œconomy it happens that stimulus applied to the surface and stomach produce such opposite effects is very difficult to explain. If heat generated sthenic diathesis, why has not its use been enjoined to such a degree as to restore health in asthenic disease. Few physicians would be so lost and bewildered in theory as to advise heat in disorders of direct debility. In putrid fevers from indirect debility a gradual diminution of heat agreeably to the *modus curandi* of our author, would be most advisable; but if we may depend on the testimony of others, washing the body with cold water is not only innocent but salutary.

lutory. We are acquainted with no disorders which may be said to be the genuine and sole effect of heat, except burns and scalds, which are the effect of the topical application of heat. They do not arise from a degree of heat barely above moderate, but from such a degree of heat as never occurs in any state of the atmosphere; so that arguments drawn from their effect on the system cannot be applied to disorders arising from the heat of the air. Neither does it appear to me, that the operation of culinary heat is similar to the heat of the atmosphere. We shall not enter into the dispute respecting the nature of fire, whether it be elementary or the effect of frictions. We do not conceive that the determination of this point would reflect any light upon the effects of heat on the human body. But we will observe, that heat simply considered, does not prove hurtful, so much on account of its altering the temperature, as in generating putrefaction in vegetable and animal substances, and thereby polluting and decomposing the air.\*

From a circumstance related in the medical Commentaries of Edinburgh, of Doctor Monro, confining a number of persons in a Bagnio, without suffering any great inconvenience until he raised the temperature to  $210^{\circ}$  of Fahrenheit's scale, it would appear that the operation of culinary fire is not so pernicious to health as the operation of an intense heat of the atmosphere. The Siroc winds which blow every year in Sicily,

\* *Vide Cullen's first lines, page 186.*

ly, are known to extinguish life instantly, & many hot days in the city of Philadelphia, are said to have the same effect when the heat of the atmosphere has not raised Fahrenheit's thermometer over  $96^{\circ}$  or  $100^{\circ}$ —To what can this difference be imputed except to the circumstance above mentioned. The effects of temperature on the human body are not accurately ascertained;—even Doctor Cullen's observations appear to me not sufficiently established. He defines the absolute power of cold, that degree which diminishes temperature, & every degree under 62, he considers as absolute with respect to the human body. I am disposed to believe however, that a degree under 62 would not if constantly applied, be attended with a constant diminution of temperature. Admit it to be a hurtful agent, the vis medicatrix would obviate its pernicious tendency, and the body would soon be accustomed to bear it. The effects of heat are when temporary confined to the skin, and do not alter the common temperature of the body. That heat unaccompanied with any other noxious agent is not very pernicious, is proven from persons sustaining a great culinary heat without any manifest inconvenience, while the burning of charcoal is often known to have the most deleterious effects. The intense heat of forges, and other mechanical employments do not produce disorders that are commonly the effect of the heat of summer. First, because it is not so constantly applied to the body. Second, because it is unaccompanied with marsh effluvia or any alteration in the atmosphere, except a dimi-

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nution of its density. Burning of powder, and other combustibles, are often used to purify the air. Any considerable diminution of the density of the air would undoubtedly be hurtful to the human body, as a certain density of it was intended by nature to assist the muscular fibres of the skin and superficial vessels to counteract the powerful force of the heart in the circulation of the blood. But a small rarefaction of the air is not attended with the hurtful consequences to health that a less rarefaction would be when accompanied with an alteration in the component parts of the air. In what respect the particles of light that emanate from the sun differ from the particles emitted by a candle or culinary fire, I will not attempt to explain—but that they have different effects on the system is to me a matter of firm belief.

Paragraph CXV. *Heat in extreme excess constantly debilitates. Its effect is somewhat greater upon the skin than in the interior parts, in which there is little change of temperature. Hence arise sweating, as in the torrid zone, &c.*

That great heat should diminish tone very little except in the skin, while a degree above moderate should increase the tone and density of the muscular fibres, every where appears to me a little astonishing. Whatever effect heat has upon the muscular fibres generally, must be in consequence of a sympathy existing in all parts of the body. And although I do not believe that

a change in the temperature of the skin is so readily communicated to the internal parts, as a change in the condition of the stomach is communicated to the skin, or other parts ; yet I would suppose that if a small change in the temperature of the skin would be communicated to the distant parts a greater change would be more speedily and effectually communicated. The author in account of temperature, seems to be something inconsistent. For in a succeeding paragraph he says, the same agent (meaning heat in extreme excess) *in the violent measles, in the confluent small-pox, in fevers, and in every asthenic disease, in which the perspiration is deficient, though it expands the vessels increases deficiency of perspiration.*

How the same power produces two opposite effects, the author has attempted no explanation. If, however, health and disease are the same states, the effect of heat in health and disease would be the same. But as two opposite effects are induced by the same degree of heat, we must infer that health and disease are not the same. Heat and cold are relative terms, and it would be well if we could fix upon a middle salutary point of temperature, from which to reckon both the morbid and salutary effects of heat and cold. It is a task beset with many difficulties. Our author's attempt is very indifferently executed, and although my observations on temperature may not appear very methodical, I hope they contain some useful truths. Doctor Brown has certainly misrepresented the effects of temperature on the human body. The degree of heat above moderate, I cannot suppose to generate

sthenic diathesis. The known healthiness of the inhabitants of northern climates, and especially in the coldest weather, does not correspond with his imputed effects of cold.

Paragraph CXX. *As cold is naturally debilitating, and all debilitating powers diminish excitement, it is therefore never of service but in sthenic diseases.*

The temperature of heat and cold, as long as it keeps within the range of agreeable sensations, is no doubt salutary. But when it exceeds that range considerably, and is constantly applied, it then becomes hurtful. As the sensation of cold or heat however, is greatly diversified, owing to the sensation immediately preceding, it is difficult to establish what degree of temperature is agreeable. We are the creatures of habit, both in our moral and sensual feelings, and a degree of temperature that is agreeable to day, may tomorrow become disagreeable. The extremes of heat and cold produce pretty nearly the same effects on the human body. But if we fix upon a middle point between two extreme opposite points, I doubt whether a diminution of temperature, by which we mean cold, would be so hurtful as an increase of temperature. The intermediate degrees of temperature in their effects, do not approximate so nigh one another as the two extremes. Arguments drawn from the effects of cold on vegetable life and the elements, very illy apply to the living system: For

as the human body contains a power of generating heat within itself, a degree of cold that would extinguish vegetable life and destroy the form and consistence of the elements, would, by proper successions and alternation with culinary heat, be salutary.

Experiments have discovered that some kinds of air that are the most powerful in extinguishing the vital principle in animals, increase and cherish the growth and perfection of plants.—The different organization of animal and vegetable life, modifies the operation of temperature. Cold has a different operation on the brute creation from what it has on the human species. Animals enjoy better health in the summer, are more fat and plump, while the human race emaciate, and are more diseased than in winter. Whether this may be imputed to the alternation of cold with heat, or to a weaker power in generating heat in other animals than in man, I will not pretend to determine. The fact that some vegetables live all winter, proves that the noxious or salutary power of cold depends on organization as well as habit. A fact that proves cold less prejudicial to health is, that nature in the formation of man, did not limit the vital principle in generating heat. Whenever the vital principle is strong, a great quantity of heat will be generated, and even when it is apparently weak, a nifus will be exerted to restore the health of the system. The constitution of man being thus formed, it was necessary that some external

external agent should exist whenever redundancies of heat were accumulated to refrigerate the body and reduce it to the healthy standard of temperature. In atmospherical air, we find an agent every way suited to produce this effect: and I agree so far with Doctor Brown as to believe in the utility of cold in sthenic diathesis, but I do not agree to its being so dangerous in disorders of direct debility. Doctor Cullen observes, that cold manifestly possesses a tonic and astringent power in its operation on the body. He also observes that every change of temperature from higher to lower, proves more or less stimulant, except it is so intense as to extinguish the vital principle in the part. The first effect of the application of cold, he observes, proves both astringent and stimulant. These observations are founded in truth. Even in cases of direct debility the application of cold when not long continued, is salutary. Convalescents recovering from fevers of every kind scarce ever return to their former vigor, until they have experienced the stimulant and tonic effects of cold. Cold bathing has been recommended in intermittent fevers and the debilitated state of patients recovering from every disorder. And although it may be well to be governed in advising this mean by the causes which produced the disorder, and the species of debility that exists; yet the hurtful effects of cold I am not very apprehensive of, except a degree of cold below  $62^{\circ}$  and that degree constantly applied, or some obstructions exist in some of the viscera.



In short the sedative effects of cold so much feared by Doctor Brown, and spoken of by Doctor Cullen, as taking place from every degree of absolute cold, appears to me to exist in idea rather than in fact: For except cold extinguishes the vital principle; induces a palsy or gangrene, all the other effects evidence a stimulant operation.

Doctor Cullen, in his account of the sedative and stimulant power of cold, delivers what appears to me an inconsistency:—For he says, the sedative power of cold takes place from every degree of absolute cold; and yet, in a succeeding aphorism, he says, every application of temperature which gives a sensation of cold, whether from the absolute and relative powers of cold, is in its first effect both astringent and stimulant. I entirely coincide with Doctor Cullen, that the first effect of cold is to stimulate in every condition of the system, except in sthenic diathesis, and then if constantly applied, it manifestly refrigerates and lowers the temperature. Its refrigeration even then depends on its constant application; for if it was alternated with culinary heat in the small-pox, it would only serve to encrease the diathesis. In addition and confirmation of what I have said respecting the difference between culinary heat and the heat of the sun, it is my opinion, the reason why a colder temperature agrees better with the constitution than a hotter is, that with the former the density of the air is intimately connected, and with the latter its rarefaction.

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It appears to me that a person exposed twelve hours to a considerable degree of cold, and then exposed to twelve hours heat of a summer's day, would feel more debilitated than to have experienced a degree of culinary heat equal to the heat of the sun.

I am sensible of the want of connection in this view and account of the effects of temperature. It is a subject attended with great difficulties in its discussion.

Paragraph CXXXIV. *It is not an excess in the quantity of blood that upholds hæmorrhages, but laxity and atony from its deficiency.*

It appears a matter attended with great difficulty to establish the pathology of hæmorrhages on any general simple principle. They occur in such different conditions of the system, that I have no doubt in some instances they may be owing to debility, and in others phlogistic diathesis. Although Doctor Cullen arranges this order among his pyrexial class, we cannot infer merely from his arrangement, that they are the sole and constant offspring of inflammatory diathesis. They resemble the other orders of this class in being attended with an original fever. He supposes the spasm and fever formed prior to the appearance of the discharge. Although it appears from his explanation of the subject, that the *vix medicatrix* is instrumental in their production, I have some doubts about the originality

ty of the fever :—For in the same order he has arranged pthilis pulmonalis, the fever of which he denominates symptomatic. The foundation of this distinction is not so obvious. Let the fever be primary or symptomatic, a partial debility or unequal excitement seems necessary to precede their appearance. The effects of food and the other non-naturals on the animal œconomy often elude investigation. It is possible that the non-naturals may have some agency in producing a partial debility, but if so, it is in such an insidious manner as to escape medical sagacity. The parts subject to hæmorrhages by not being covered with the same thickness of skin as other parts of the system, might naturally be offered as a reason for their so frequent occurrence in those parts. They are more liable to be effected with the vicissitudes of the air. But we would presume a priori, that the coats of the vessels of those parts would have been formed more dense, to resist an influx of blood sufficient to produce a rupture. Experience, however establishes that they are more subject to hæmorrhages than other parts, and we may justly impute their frequent appearance to this cause. Hæmorrhages very seldom, if ever, occur in infancy or advanced age. They generally happen at the period of commencing manly vigor, when large quantities of food are taken in, and the digestive organs are vigorous. They may possibly occur in subjects labouring under debility, but it is my settled, rooted opinion, that it is a rare occurrence. Why do they not happen in debilitated states of  
fevers,

fevers, dropſy, and other diſorders of debility? They ſometimes happen in the firſt ſtage of a fever, and I have generally remarked that their occurrence at thoſe times was ſalutary; whereas, if they were the offſpring of debility, they would have been morbid. Although diſcharges of blood do frequently happen in different parts of ſcorbutic patients, I have never known of an hæmoptiſis occurring in that diſorder. Why do they not happen in patients recovering from fevers, when a greater apparent debility exiſts than when the fever rages with the greateſt violence? I impute it to the ſtrong inflammatory exertions of the animal œconomy. Let the opinion of the faculty be what it may, I am confident that in every paroxyſm of a fever, the ſtate of the ſyſtem reſembles inflammatory diathēſis. What adds conſiderable weight to the opinion that hæmorrhages depend on a ſthenic ſtate of the ſyſtem, is that they are apt to produce a plethora of the ſyſtem. I have ſeen young girls labouring under an hæmoptiſis, exhibit ſymptoms of the higheſt health, a florid, full countenance, and every mark of redundancy of blood. In conſequence of this plethoric ſtate of the ſyſtem, a periodic diſcharge would every now and then happen. This plethoric ſtate of the ſyſtem Doctor Cullen accounts for very ingeniouſly, by ſuppoſing that the exhalants, ſecretory, and excretory organs, in conſequence of a diminished quantity of the maſs of blood, are not filled with their proper fluids. The veſſels collapse, become rigid, and reſiſt a  
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very copious entrance of fluids, whereby they are detained in the system and a plethora follows, and consequently a discharge becomes natural and habitual. They occur in seasons of the year most apt to generate an abundance of blood. As far as I have been able to learn, they more generally occur in the spring season than any other period; and from this circumstance we may infer, that the tonic power of cold, increasing the tone of the muscular fibres has, by invigorating the digestive organs, accumulated a copious quantity of blood in the system.

The warmth of the spring, by destroying in some measure the elasticity and density of the air, and that by impairing the tone of the solids and vessels of the part, gives occasion to their appearance. Perhaps (as Doctor Brown says) the tone of the vessels of the skin is increased during the winter more than any other part, they may retain their vigor and tone in the spring longer than any other part; and as there happens to be an accumulation of blood in the system, and as the vessels of the skin resist its influx into them, it must be determined into vessels possessing less resistance. Women who from sudden colds are troubled with a suppression of the menses, often have a vicarious discharge of blood from the lungs; and as the catamenial discharge when suppressed, is a redundant quantity of blood detained in the system, a plethoric state must naturally be supposed to exist: and this affords a strong argument against debility as the



the cause of hæmorrhages. I will not answer for the pathology of Doctor Cullen, but I deny the truth of Doctor Brown's.

What agency the *vis medicatrix* has in their production, I will not determine; but if it has any agency, it is less conspicuous than in fevers, and especially as Doctor Cullen has pointed out no remote causes to induce debility or increased tone, the exciting causes of the operation of the *vis medicatrix*. It is a settled opinion of mine, that the exertions of the *vis medicatrix* are always determined from the centre to the surface, and almost always in increasing some evacuation. We have, however, heretofore admitted that the cold stage of fevers was a part of the operation of the *vis medicatrix*. We meant it only as a necessary mean to excite the action of the heart and arteries, to restore the debility existing on the surface. And we will not say that a spasm in hæmorrhages may be formed on the surface, on purpose to excite the action of the heart and arteries to throw off by the excretories the redundant blood accumulated in the system. The failure of the *vis medicatrix* must be imputed to the obstinacy of the spasm determining the blood to the lungs, or some part possessing less resisting energy. The disciples of Doctor Brown must not expect the *vis medicatrix* to be always successful in its efforts. If it is successful in a majority of cases only, it is sufficient to establish the principle. One case of hæmoptysis occurs to me, which cannot be imputed to the  
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agency of the *vis medicatrix*. Persons in apparent health have been said, upon ascending the Peak of Teneriffe, to be suddenly attacked with an hæmoptysis. Can the rarefaction of the air on that ætherial mountain occasion such a sudden loss of tone and vigor as to excite the *vis medicatrix* into action?

The stimulant plan of cure in hæmorrhages, I entirely disapprove. They encrease the morbid activity of the vessels; increase the local inflammation which always exists in hæmoptysis, whatever be the state of the general system. Whenever a discharge has become so abundant as greatly to weaken the body, we are necessitated to have recourse to stimulants and tonics. And when, from the long continuance of the discharge, the pulse becomes small and nature begins to flag, the necessity of their administration is obvious. But in the beginning they are exceedingly detrimental.

Practitioners have been deceived with respect to the utility of stimulants in hæmorrhages. When the patient has become weak and emaciated, stimulants have seemed to restore vigor, and an abatement of the symptoms have followed. But it is only to return with more vigor. I consider it as a deceiving appearance both to the physician and patient. It cherishes hope, the last comfort of the wretched, only to depress them the more. This idea is substantiated by the evidence of Doctor Brown himself. He re-  
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lates a case of a person under the cure of one of his pupils, labouring under a pthisis, who by a voyage to Lisbon and the administration of stimulants, received a cure. But two years afterwards death overcome the skill of the physician. The same may be said of the stimulant practice in pthisis pulmonalis. I have uniformly seen that practice not only fail, but often attended with an aggravation of every symptom. It increases the stricture of the breast, aggravates instead of checking the violence of the fever. I have always made it a rule in the exhibition of the cortex, if it did not entirely stop the fever after giving it a sufficient length of time, and in sufficient quantity, to lay it by altogether; and I always conclude, that when it does not have a salutary effect, some morbid condition exists in the whole system, or in some part of it except debility. As the nervous influence seems to pass from the brain to the extreme parts, perhaps a solution of continuity may interrupt its progress, raise a commotion in the part, and thereby impair or change the operation of stimulants. I hazard this conjecture, however frivolous it may appear to the disciples of Doctor Brown.

One chief circumstance affecting the operation of medicine in hæmoptysis, is the inspiration of air into the lungs. The other discharges of blood are easily cured; but all the art of man has never been able to find an infallible remedy in hæmoptysis and pthisis pulmonalis. The discovery

covery of specifics, I believe is a chimerical expectation. For the condition of the body is so different in the same disorder, that a remedy applicable at one would not be equally so at another period. Man seems born to deception; they are dupes to appearances, and in no instance has it been more conspicuous than in the work under discussion.

Paragraph CLXXX. *If at any time the pulse becomes full and hard without a proportional relief of the symptoms, it is a bad sign, and happens because the stimulant plan has been pushed beyond the rule, and is a case of indirect debility superadded to the direct.*

What stimulants are we possessed of that will raise an asthenic disease instantly to the point of indirect debility? Will not the excitement in passing from one to the other, pass through the point of health? Is it not an absurdity to say that indirect debility exists during the existence of a hard full pulse? I have seldom, if ever observed such an appearance, and were it to happen, it is much easier to lessen the pulse than to raise it. This is one of the new opinions of this misleading theory. Our author often stumbles upon the dark mountains of error.

Paragraph CLXXXIX. *Wine, aromatics and volatile alkali, and above all the rest, the various forms of opium, dislodge from its seat all such hurtful matter, without either vomiting or purging, and without difficulty, and in a short time.*

The administration of emetics and cathartics in collections of crudities in the stomach and intestines, is a practice sanctioned by the experience and authority of ages. I am confident that wine and aromatics, and especially opium, instead of discharging crudities in general, serve to aggravate and procrastinate the disease. We may aptly here apply an observation of Doctor Cullen in speaking of the effect of cathartics in discharging hydropic waters, that if they do not suddenly have that effect, they increase debility. This is a general principle pervading the animal economy. Opium, on account of its constipating the bowels, seems the most improper of all stimulants. It suddenly abates pain, and where no crudities are collected, and the pain depends on simple debility, it may by repeated exhibitions effect a radical cure; and I imagine it has been cases of this kind that our author has mistaken for collections in the elementary canal. And although I agree with the author in supposing them the effect of debility, I cannot believe in simplifying medicine so far as to raise but one indication of cure. The muscular fibres, as the author says, differ from common elastic matter in this; that they contract not only when the distending power is removed, but even while it remains; But they do not contract with that facility in the time of distention, as when the distending cause is removed. Ineffectual efforts in the muscular fibres are often made to contract, and the consequence is an encrease of debility. And although I may not be able to explain why stimulants fail



of discharging the crude contents of the bowels, I am certain of the fact, and no theoretical reasonings will ever induce me to give up this belief. Crudities collected in the primæ viæ may prevent medicines coming in contact with their muscular fibres. An unequal excitement undoubtedly exists in those cases, and the failure of stimulants may be imputed to this source. I am clearly of opinion, that to the successful administration of tonics and stimulants, a uniform exertion of the vital principle is absolutely necessary. It is also necessary that previous to their exhibition the secretions and excretions should be restored to their usual quantity.

It may be a questionable point with some whether stimulants or diaphoretics are the most certain to restore the excretions. But experience has confirmed me in the belief that medicines of the latter class, whether they effect a cure by a stimulant or sedative operation, are more effectual than medicines without this property possessing a higher stimulant power. And what strengthens this opinion, and especially the observation that the success of stimulants and tonics depend on the uniform exertions of the vital principle is, that I have always observed their exhibition to be most successful when perspiration was restored, and the patient entirely free from pain. Although spasm (as the author says) may be a deficient function, yet as it is a visus to discharge the crudities, or to restore the debility, the operation of stimulants often aggravate the

the pifus without effect and increafe the debility. The activity of the fibres are often too vigorous, and like the reaction in fevers, require to be abated previous to the exhibition of tonics.— Since Doctor Brown's fyftem made its appearance, I have in fome instances attempted his mode of practice, but the fuccefs which he flatters us to expect, entirely failed me in the trials I have made. I had under my care a child laboring under a diarrhæa from weaning, and from the emaciated ftate and age of the patient, it appeared to be a cafe in which the ftimulant method of cure bid as fair to be fuccefsful, as any that falls under common obfervation. Previous to my determination to exhibit ftimulants, I had ufed Rhæi without any effect. I then exhibited brandy, and to make it a little more ftimulant, I added a fmall quantity Canella alba, with the fame fuccefs. I then began to exhibit liquid laudanum twice a day, increafing the dofe from day to day, but without the leaft abatement of the difeafe. Difheartened in the ufe of ftimulants, I reforted to an emetic. Immediately after the operation of the emetic the ftools became lefs frequent and of a thicker confiftence. Encouraged with the fuccefs of the firft, I exhibited a fecond emetic. It operated in a moft violent manner, both upwards and downwards, fo much fo, that from the emaciated and debilitated ftate of the patient, I began to fear the roughnefs of its operation might be dangerous, but with the exhibition of a few drops of laudanum the vomiting and purging ceafed, and from that time the

the diarrhœa abated, the child recovered appetite and flesh, and the cure was complete and effectual. From this and other cases which have fallen under my observation, I am perfectly convinced of the impropriety of following Doctor Brown's general *modus curandi*. Cases may exist where no crudities are present, and where the *vis medicatrix* may be wearied so that the fibres become almost entirely passive, and in which his mode of cure may sometimes be successful. I have seen alarming appearances in the dysentery from the continued exhibition of opium. And what would be the consequence if his practice was universally imitated. Pain and spasm do not always arise from debility, together with a distending matter. A state of emptiness in the stomach and intestines, occasions the painful sensation of hunger.

Paragraph CXC. *Pain in the external parts, also depends upon spasm; but not with the conjunction of distending matter, and a power takes the place of it, which is not to be referred to any matter, but to a certain effort of the will in moving a limb.*

In what manner an effort of the will can act as distending matter, is to me entirely incomprehensible. Can it be an instinctive exertion of the will to remove pain, which is conspicuous in patients tossing and changing their position, when labouring under spasmodic affections? This happens after the commencement of pain, and there-

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fore cannot be said to operate in its production. This would countenance one of the greatest philosophical absurdities, that the effect existed before the cause. From the known effect of pain the author infers that the will operates in the external muscles in the production of pain, as a certain portion of excrement would were it to exist in a muscular fibre. Fanciful indeed is the supposition, and (as the author says) contemptible are the resources of ignorance. It may cherish in the mind of the author a strong opinion of the novelty of his work; but it will answer no useful purpose in the illustration of the pathology of disorders.

In the same paragraph we have the following remarkable sentence—“*Spasm in the external muscles is removed by restoring strength; for that reason the cause also must be the same and be reducible to debility, together with something that resembles debility and possesses power equal to it.*”

What effort of the will resembles debility? Can debility be compared to any thing in nature except the relaxation of a simple cord? Strong exertions of the will produce debility. But the cause and effect have no resemblance. We associate cause and effect; but the association is not from resemblance, but as prior and posterior.

Paragraph CXCI. *The pain of cholera arises from a concentrated acid predominant in the alimentary canal.*

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That there constantly exists a portion of acid in the alimentary canal, and that the quantity may be increased in a debilitated state, will be readily admitted. In cases of an increased quantity of acid in the primæ viæ, its power of stimulating the fibres in a given quantity may be enlarged; but we have no positive proof of it, for the pain may with great propriety be ascribed to the enlarged quantity, not to any concentration of the acid. It is incompatible with the idea of a concentrated acid to suppose the quantity increased. A concentrated is a diminished quantity of acid, possessing the power of the quantity from which it is distilled. The debility of the intestinal canal, does not operate in the production of acid any way similar to chemical operations upon acids subjected to chemical process. If it is necessary to distinguish this from other acids, it would bear the name of a diffused better than a concentrated acid. In this disordered state of the intestinal canal stimulants and tonics, often fail after the evacuation by vomit and stool, have proceeded so far as greatly to debilitate. A powerful constriction may exist on the surface, determining the fluids with great impetuosity to the stomach and intestines, to which may be imputed the failure of stimulants. For we are often necessitated to have recourse to external means, as epispastics and warm bathings before stimulants have a salutary effect. I have seen cases of cholera, after the strongest stimulants failed, give way to Magnesia and Columbo, substances which possess no great stimulant power. They act as absorbent, rather than stimulant. Numerous examples might be



adduced to disprove the notion of one indication being sufficient to effect a cure in disorders of debility.

Paragraph CXCIV. *As an acid produces pain in the internal parts, so in the external parts, it is occasioned by something that produces the same effect as the acid that depends upon the will and acts in conjunction with the convulsive state.*

What is that something that depends upon the will, acting like, or producing the same effect as an acid? If it depended on volition, why are we not instructed how to remove it by an effort of the will? I cannot conceive of a modification of the will acting like an acid in the production of pain. It is as mysterious and enigmatical as the Sybilline or Delphian oracles. I will not admit that he has discovered upon scientific principles, a certain mode of cure for one half of the disorders of the human race, while former physicians have universally failed. I really wish to imbibe a small portion of this effulgent discovery, but whether it is to be imputed to the dullness of mental vision, or renitency to unlearn what I have already learned, I will not, cannot determine.

If the author has any disciples in America, I call upon them to develope and illustrate principles so important to the human race. In his review of morbid affection, the author traces the symptoms in the order in which they make their  
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appearance. Want of appetite, nausea and vomiting, are by no means the certain forerunners of every disorder. Disorders often exist without any apparent change in the state of the stomach. The effects of drinking cold water are as various as the different conditions of the animal œconomy. I cannot suppose it very debilitating; on the contrary I have generally observed it to have a tonic effect upon the muscular fibres of the stomach, and by its sympathy with other parts, a like effect upon the general system. In fevers, physicians have generally been cautious of a too free indulgence in draughts of cold water, but I imagine the caution did not proceed from its debilitating operation. If we may depend on the reports of others, I have no doubt that cold water has effected an entire solution of fevers. The mode of its operation may be mysterious, but I am sure it is not by increasing debility; *the cause of fevers.*

The fears of practitioners in the use of cold water are every day diminishing. Their fears have often been groundless, and I am happy to see this symptom of freedom of thought and inquiry among the faculty, who have heretofore been governed more by whim and caprice than sound judgment and reflection. The operation of cold water is most doubtful in pneumonic inflammation. It has been observed to occasion pleurifies when taken in large quantities, and especially when the circulation has been quickened, and ~~the~~ heat of the body has been raised considerably.

bly above its ordinary temperature. This effect could not be imputed to the sedative and debilitating operation of cold water. The faculty in the cure of the colic, have generally been in the habit of administering cathartics, and especially when attended with constipation of the intestinal canal. Colics are sometimes of an inflammatory nature, and at others they appear to be the offspring of debility. And as those two different conditions of the body would not alike be removed by the same means; an uniform administration of stimulants would not be justifiable nor successful. Whenever the healthy tone of the intestines is impaired, costiveness or laxness generally succeeds. And when spasm, pain and costiveness have happened together, physicians, without any regard to encrease or diminution of tone, have began the cure by an evacuation of the contents of the bowels. And although Doctor Brown says, that an invigoration of the intestinal canal will effect a certain cure without this precaution, I doubt the truth of the assertion. In slight affections of the bowels unattended with costiveness, an opiate may affect a radical cure. But when the belly is obstinately costive, no invigoration of the peristaltic motion will enable the intestines to discharge their contents. Admitting stimulants would have this effect, it appears to me that premising a cathartic would expedite instead of procrastinating a cure. The distention of the fæces oppose the contraction of the intestinal fibres, and as it is by contractions their healthy vigorous state is restored, the spee-

diest way will be to evacuate their contents, that the muscular fibres may have free liberty to contract. Doctor Brown certainly did not urge the use of stimulants so much with a view to save time, as to impress the world with the amazing strength and fertility of his genius and novelty of his system. From manifold experience, I am convinced that opium, although it abates pain in colics, yet as it encreases costiveness, after its operation is taken off, it disposes the intestines to fall into spasms, and an effectual cure scarce ever happens, except the bowels are first evacuated. Nor have I more faith in the use of stimulants in tabes, atrophy and worms. Atrophy and tabes are sometimes owing to obstructions in the mesenteric glands, and sometimes I believe they may be imputed to an abundant quantity of mucus lining the intestines of children, closing up the mouths of the lacteals and thereby preventing the absorption of nutritious matter. But to whichever of these causes they are owing, I doubt whether stimulants possess such extraordinary power as to remove these complaints in a few hours, days or weeks.

An atrophy or tabes from obstructions in the mesenteric glands, is a disorder of the most confirmed obstinacy. I have very little experience in either of these disorders, but the reports of others, on whose veracity I rely, inspire me with a belief that they are very little under the power of medicine, and least of all under that class of medicines called stimulants, they confirm the obstruction and aggravate the disease. When  
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these complaints originate from mucus in the intestines, stimulants may by exciting vigorous contractions of their fibres, hasten their discharge and consequently a cure. But opium, one of his diffusible stimulants certainly diminishes the activity of the muscular fibres and would therefore appear a very improper medicine in this and many other complaints in the use of which Doctor Brown is so liberal—I have the same doubts about the use of stimulants in worms. A small use of spirits or some stimuli are of use to keep up the tone of the bowels of children—they are disposed to be lax and flaccid, and their secretions are greater in proportion to the mass of blood, than adults ; but I am positive that a continued use of stimuli for weeks and even months, would not dislodge these vermin so effectually as Dianthis and calomel in a few days. And I am confident that physicians will not give up a sure and certain cure in Dianthis and calomel upon the *ipse dixit* of Doctor Brown. Worms are more rare in Europe than in America—And whatever purpose stimulants may answer there, I am sure it would be madness for the faculty in America to lay aside a sure and effectual cure for one that stands on the feeble foundation of an individual's word. We may and very often do have recourse to stimulants and tonics to brace and fortify the bowels against their future generation.

Paragraph CCVI. *There are four kinds of inflammation, two universal, a sthenic and an asthenic, two local a sthenic and asthenic.* The



The asthenic inflammations are creatures of imagination ; they do not exist in *rerum naturæ*. Inflammation conveys an idea of a tense elastic muscular fibre through the whole system or in local inflammations a vigorous action of the vessels in the part and an abundance of blood in the habit. The idea is in direct opposition to an asthenic state. The epithet asthenic when conjoined with the word inflammation is incongruous and incompatible—they import contrary ideas. The adjective should always agree in sense with the substantive to which it is affixed. Inflammation is synonymous with increased excitement—It is as absurd to say asthenic inflammation, as to say commendable or good vice. They convey an idea of two opposite conditions of body existing at the same time which is impossible. Our author possesses a happy faculty of reconciling ideas which in every other person's mind stand in direct opposition. The affection which he denominates a general asthenic inflammation in the gangrenous fore-throat agrees no further with inflammation than in the bear redness of the fauces. The vessels of the part appear languid and inactive ; they manifest no appearance of increased vigor ; there are no forcible contractions of the fibres ; no violent pulsations of the arteries appearances, essentially necessary to constitute inflammation. In a former part of his work he has asserted that all the symptoms are participant of the nature of the diathesis. The diathesis in the putrid fore-throat is asthenic, and as the word *asthenia* is opposed

posed to inflammation, therefore an inflammatory state depending upon the diathesis cannot exist. The fluids in this disorder are determined to the throat in larger quantities than any other equal part. In what manner the effluvia of this complaint affect the throat more than any other part of the system is inexplicable. Can we suppose that the first application of the contagion is exerted upon the fauces, and from thence communicated to the system? Or do they exert an operation upon the whole surface of the body? Is the more violent affection of the throat owing to the want of the cuticle? He advises the exhibition of powerful stimuli to drive on with impetuosity the blood which loiters in the affected part, but if the medicines administered had not the effect to brace the whole system and the vessels of the affected part in equal proportion, they would urge the blood only to increase the debility of the vessels of the fauces. For the first days of this disorder it often exhibits an inflammatory appearance, but it is a deceitful appearance—the pulse does not shew that vigor that is so conspicuous in pneumonic inflammation; and as an inflammatory diathesis, must always precede a state of indirect debility, and as the first stage does not shew an increased excitement sufficient to induce indirect debility, we cannot infer its existence. In states of indirect debility suddenly brought on while the vessels are yet full, I am convinced of the propriety of venesection and other evacuations, thereby to give the vessels liberty to contract. Venesection is rarely admissible

sible in a complaint so strongly marked with debility and putrefaction. I can see no reason for supposing this complaint symptomatic, any more than the cynanche tonsillaris. They are both significative of the diathesis on which they depend. I agree with the author in believing this complaint improperly arranged in the systems of nosology. The affection of the throat, and the general condition of the body is so different from the other affections of the throat with which it is arranged, that its classification appears entirely preposterous.

Paragraph CCXXV. *The burning sense of heat on the palms of the hands and soles of the feet, is owing to debility checking perspiration, because fatigue, cold and other debilitating powers are hurtful, and heat, rest and stimulants are salutary.*

This painful sensation is undoubtedly owing to debility, because I have always experienced whenever that affection came on, that cold air always removed it. Cold I conceive operates in this instance by diminishing heat and bracing the weakened parts. Fatigue and heat always increases this sensation. Whatever effect cold may have in the natural state of the body, I am sure that when an extraordinary degree of heat is generated in the whole system or any particular part, whether from causes increasing or diminishing the tone of the fibres that cold by refrigeration diminishes the heat and restores the healthy state.

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Paragraph CCXXX. *If fever, if the gout in digestion, colic, asthma and all asthenic diseases have lately, to the conviction of every person who gave the subject a due consideration been proved to yield to the various forms of opium without difficulty can we suppose it proves of service by a debilitating operation?*

The operation of opium is involved in great difficulty. The notion of its being stimulant is not the offspring of Doctor Brown's inventive brain. Doctor Cullen supposes it stimulant in its first operation, and Doctor Dickinson in the latitude of our author. I have always considered an increased momentum of the pulse as a sure sign of stimulant operation and increased velocity with a diminution of momentum as a sure token of the operation of debilitating powers. In a short time after the exhibition of opium, sleep is induced, the animal functions are suspended, and many strong marks of deficient excitement appear. The force of the vital functions appear to be enlarged; the pulsations of the heart and arteries are more forcible, but whether the enlarged momentum will compensate the deficient velocity is a question yet to be investigated. The short space of time that intervenes between its exhibition and sleep and other marks of deficient excitement rather manifest a sedative than a stimulant operation. Opium possesses a greater power of diminishing the irregular alternate contractions of muscular fibres than any other substance in the *materia medica*. From thence it

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has been denominated an antispasmodic. Former physicians contented themselves with the knowledge of this fact without enquiring into its nature and mode of operation. Is this a specific power of opium, or are we to ascribe it to a stimulant or sedative operation? Spirits and several other stimulants possess an antispasmodic power in disorders of the alimentary canal, but I am sure that no substance will as readily take off the spasms of the remote parts of the system as opium. Great marks of debility appear after the spasms have been taken off in spasmodic disorders. Would this happen provided opium operated as a directly stimulating power? If its stimulant power was in exact proportion to antispasmodic effect, health, vigor, appetite, and cheerfulness would follow its use. But on the contrary we observe nausea, languor, fullness and every mark of diminished excitement. This may be imputed to indirect debility—But I never can have faith enough to believe that one, two or three grains of opium, possesses so much stimulant power as in disorders of debility not only to raise the excitement to the point of health but to exceed that point, and continue the excitement until indirect debility commences. We are in possession of few if any medicines of such sovereign efficacy. In a system of principles established with such geometrical exactness as the present, we would expect in disorders of debility a sufficient quantity of opium to restore health only exhibited. The author should have established a scale of stimulant operation to complete his system.



system. We should distinguish substances from one another by their quantity of exciting power, not by taste, smell or botannical relations. After the use of opium in fevers and in all spasmodic disorders, we are necessitated to have recourse to tonic and stimulant medicines to complete a cure. In the asthma and epilepsy it may shorten a paroxysm but never in any quantity or exhibited for any length of time, does it produce a radical cure. Opium exhibited in the paroxysm of an intermittent, will hasten the solution of the paroxysm, but never by a stimulant operation prevent their recurrence. It is sometimes exhibited a short time before the expected paroxysm, but I expect if it has a good effect it is by blunting the sensibility of the system. If sleep is a mark of diminished excitement, if costiveness is a mark of languid action of the muscular fibres, of the intestines, and if these phænomena universally succeed the administration of opium by what strained inference can we make opium a stimulant—If a stimulant, why does it not invigorate the peristaltic motion to evacuate the alimentary canal?

The hurtful effects of opium in inflammatory disorders I expect will be urged as a proof of its stimulant operation. I have administered it in pleurisies, and I must acknowledge with manifest injury. But whether the injury is to be imputed to stimulant operation or to a diminution of activity, may be a questionable point. The healthy tone of the fibres depends greatly on a repetition

tion of contraction, and on a due quantity of nutritious chyle. And if it can be proven that opium effects neither of these ends, but on the contrary diminishes the contractile disposition of the muscular fibres, its stimulant power must immediately become doubtful. Exhibited in pleuritis it abates pain, whereas, if stimulant, we would expect as the pain depends on the diathesis, it would be increased. In pleuretic complaints, nature attempts to relieve the fullness of the system by a discharge of mucus and blood from the lungs. Exhibit the smallest quantity of opium, it instantly checks the cough and expectoration, whereas, if stimulant, it would increase the cough. The small quantity that will stop the cough and expectoration, I presume would not suddenly induce indirect debility.—Ardent spirits and other stimulants increase and aggravate both cough and pain, from which we conclude that the operation of opium is no ways analagous to general stimulant powers. A small quantity is observed to occasion wakefulness, while a large one generally brings on immediate somnolency. It is to be noted that a large quantity of any exciting power proves sedative. A small quantity of food stimulates, increases activity, while a large one overcomes the action of muscular fibres and increases deficiency of excitement. It is on this foundation that opium is generally exhibited. It suspends the contractile power of muscular fibres without raising the excitement to the point of indirect debility. It requires time and a gradual and repeated use of moderate

moderate stimuli to raise the excitement to 70°. And I believe it never happens from the use of medicines, without a gradual invigoration of the digestive organs, thereby enabling them to convert the food into a large quantity of chyle, and by the other functions of nature applying it to repair the waste and increase the fulness of the system. The tone that medicine gives to muscular fibres, is entirely different from that arising from the use of nutrient substances. It is observable that people never recover their former tone and vigor until their appetite returns. And I have seen cases of evident debility, where medicine (as opium for instance) seemed to blunt the sensibility, or, in the language of Doctor Brown, to wear out the excitability, and impede instead of hastening a recovery. I do not conceive that the free use made of opium by the Turks to urge them on to battle, is a very powerful argument in support of its stimulant operation. Fear is often connected with great mobility; and we will acknowledge that opium lessens the mobility during its operation. But that it answers this end in the same manner of other stimuli, is questionable. It takes off the resistance of the vessels to the influx of their fluids, diminishes the velocity of circulation, retards the return of the venous blood from the head, and in this manner we are to account for the passive courage of the Turks in battle. The Turks do not possess the ferocious courage of the American Indians. They starve themselves to whet their courage, and they are more alert

and active than an army of Turks under the enfeebling operation of opium. I place no reliance on its stimulant power, because it sometimes occasions cheerfulness of mind. I have experienced the greatest cheerfulness of mind, and felt the most firm and courageous when I have been under the operation of powers manifestly debilitating.

It is a universal property of stimulants to extend their influence to the remotest parts of the system. Opium and ardent spirits, soon after exhibition occasion a coldness of the extremities, a mark of sedative operation. If opium possesses a power of stimulating above the ordinary exciting power necessary to maintain health, its operation is very transitory and less durable than other stimuli. I will not contend that opium stimulates none at all, for it certainly in some cases manifests stimulant operation. But whether those appearances are to be ascribed to its exciting the action of the *vis medicatrix* (agreeably to Doctor Cullen's opinion) or to a direct stimulant operation, I will not determine. I will observe however, that the phenomena of stimulant and sedative operation exist together; their boundaries are not so well marked and defined as other stimuli, and that if stimulant, its operation is *sui generis* and not analagous to any other substance. The homogeniety of operation of every substance in nature, as asserted by Doctor Brown, appears to me doubtful.

The assertion of the author that opium is the  
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most powerful medicine in supporting the watching state, is altogether incredible, and requires a degree of faith equal to the novelty and falsity of the assertion. There are persons that are remarkably wakeful under the operation of opium, whether from the smallness of the dose, or an idiosyncrasy of body, is not always very apparent. The common effect of opium is to induce sleep. It is exhibited with that view by every physician since its first introduction into the materia medica. They could not have been mistaken in a fact of such universal notoriety. Doctor Brown might as well attempt to reason physicians out of their senses. The operation of opium, like every other substance, is various, depending on the condition of the system at the time of exhibition. This was known to the faculty, as well as that opium increases some pains, before the appearance of the new theory. Cathartics the most violent in their operation, are suddenly checked by the administration of opium. And as cathartics, although their evacuation occasions debility, yet as their mode of effecting evacuation is stimulant, we would suppose that opium instead of interrupting would increase the evacuation of the bowels. We hold it as a medical truth, that all stimulants operate by increasing the contraction of muscular fibres. Opium therefore as a stimulant, should increase the contraction and activity of the intestines and hasten the expulsion of their contents. But as it evidently has a directly opposite effect, we presume that the operation of opium in this instance is not stimulant.

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In a note upon Paragraph CCXXXII, the author says that *excitability is one uniform, undivided property over all the system.*

That a certain portion of excitability is assigned to every part of the living system our author acknowledges, and also that a greater quantity may be assigned to one part than another of equal nervous importance. After this explicit acknowledgement of the different degrees of excitability in different parts, with what propriety can he infer its uniformity? The word *uniform* carries an idea of equality in quantity in every part of the system. The property of indivisibility ascribed to the excitability by the author, is to be doubted. For as the excitability is inherent in *medullary matter*, and as when a limb or member is taken off, some portion of medullary matter must also be taken off, the divisibility of the excitability therefore follows as an irresistible conclusion.

The excitability in its nature, like immaterial beings is indivisible. But in effect if it is inherent in medullary matter, it is often divided, as in gangrenes, loss of limbs, &c. If the excitability was generally and uniformly diffused over the system and had a ready instantaneous communication, why would not a medicine in contact with any one part, extend its effects to every other part, and in case of disease a complete cure be established. But the excitability is not uniform. A wound in the root of a vegetable

getable, or in the head of an animal, is followed with worse consequences than a wound of equal extent inflicted in any other part. In the mind the excitability is subject to alternate periodical increase and abatement, while in other parts it for the most part remains stationary.

Paragraph CCCV. *The cure of any sthenic disease is improperly trusted to bleeding alone, because that although that operation may reduce the excitability in the greater blood vessels, perhaps too much ; yet in the extremities of these as well as in the rest of the body it is not sufficiently reduced.*

The operation of exciting and debilitating powers I have supposed to be very general over the system. Even the partial application of exciting or debilitating powers I expect their effect in the part is readily communicated to every other part of the system. The numerous anastomoses of vessels ; their general extension and communication ; together with the connection of the arterial and nervous systems is such that the excitement cannot be taken off in the larger vessels without immediately extending that effect throughout the animal œconomy. The operation of blood-letting has a sudden and universal effect. It is not confined to one part. The reasoning of the author does not appear like contemplating the body as a whole. I consider venæsection a powerful mean of reducing sthenic diathesis, because it *evacuates a stimulus as*  
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*much more powerful than any other, as it is directly applied to a greater extent over the system.* The vessels of the stomach, the perspiratory vessels, and all the secretory vessels participate in the effect of venesection and cathartics. The exhibition of an emetic after venesection by the consent of the stomach and skin, may effect a diaphoresis, and that a diminution of excitement greater than any other medicine.

The good effects of vomiting do not consist so much in unloading the mucous follicles of the stomach and intestines as in removing the spasm of the extreme vessels. If medicines operating on the stomach did not extend their influence over the system, cleansing the stomach would have no more effect in restoring the healthy state, than cleaning one pump-box would restore the use of a pump when both are foul. But we are more wisely organised; all the functions depend on the vital principle; every article of diet is subdued by the vital principle and made subservient to life; and every medicine acts on the vital principle; and as it is generally diffused, the operation of medicine is general and universal. We do not mean to shew in what the vital principle consists. We assert that it exerts its influence from within outwards, as it appears that the blood in all animals moves from the heart to the extreme parts.

The operation or power in the vital principle seems to be weaker in the extreme parts than the internal parts. A coldness of the extremities is  
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the first symptom of every general disorder. The exciting hurtful powers operate on the extreme sensible parts. They produce spasm, and spasm an inverted peristaltic motion of the arteries. This opinion is rendered probable by the rolling-wiry feel of the arteries in every general disorder, and especially evident in disorders of great obstinacy.

To return from this digression to the necessity of cathartics and emetics in sthenic diathesis, we assert, that on the principle of this system, they do not appear indispensably necessary. Immediately after venesection, a contraction of the vessels and muscular fibres takes place, so that if any fordes or foulnesses loiter in the emunctories, they will be squeezed out by the efforts of contraction. The excitement being equally reduced in the muscular fibres of the intestines, they of consequence must contract and expel their contents. It is to be observed that an increased or diminished excitement retards the discharge of the fœces, so that to hasten its expulsion, diminished excitement must be raised, and increased excitement diminished.

I acknowledge these observations would not be considered very orthodox on any other principle except our author's. But they are perfectly consonant to his observations on the effects of stimulants in colic and worms. If there be no necessity of administering medicines to operate on particular functions in those disorders, I am sure  
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it would be equally frivolous in general sthenic diathesis.

Paragraph CCXXXII. *While the general affection for the most part precedes the local affection and never succeeds to it.*

The exanthemata and peripneumony, although they are equally disorders of sthenic diathesis, yet the local affection of the one is entirely different from that of the other. It is certainly necessary that the fever should precede the eruption in exanthematic disorders, but in peripneumony the case is widely different. The general must either precede, accompany, or succeed to the local affection. Therefore I cannot see the propriety in saying that the general for the most part precedes, and never succeeds to the local affection. If it never succeeds it must always precede, for their coincidence is a rare and accidental (if possible) occurrence. Doctor Brown holds out an idea that the diathesis and the disorder are one and the same. A sthenic diathesis may exist without disturbance in the system, and I have seen cases of pneumonic inflammation succeed to disorders of debility and happen in persons manifestly of a weak asthenic habit of body. I therefore conceive that the diathesis and the disorder are not exactly the same, and that they often stand in the relation of cause and effect. The symptoms constitute the disorder. The symptoms flow from the diathesis. Fever, a pain in some part of the thorax, cough and difficult respiration,



piration are the symptoms of pneumonic inflammation. It is a matter of little importance in the treatment, whether the pain precedes or succeeds to the fever. To give a faithful and true explanation of the production of local inflammation would be a difficult task. But I lay it down as an axiom, that an inequality of excitement or nervous influence, must exist previous to the production of local affection. The resisting energy of the vessels of the part must be weakened, or the resisting energy of all the other vessels of the system enlarged. One or the other must happen, or we can have no possible conception of the production of local affection. Which of these events is the most likely to happen? Or if one happens, does the other follow of course? The fever in pneumonic inflammation scarce ever observes those exacerbations and remissions that happen in genuine idiopathic fevers. This strengthens the opinion that the fever is the consequence of the local inflammation, not original but symptomatic. Although the local affection sometimes supervenes to general disorders, I have observed that pain existed generally prior to much disturbance in the vital functions.

Paragraph CCCXXXVI. *The shivering and sense of cold depend for their cause upon the dryness of the skin. The languor and lassitude point out a higher degree of excitement in the brain and muscles, than can be conveniently borne by the excitability confined within certain boundaries.*

Why

Why have recourse to one symptom to explain another? The cause of all the symptoms may be sought for in the general system. If either the one or the other are anterior in point of time I should presume that a shivering and coldness made their appearance first, and the dryness of the skin was the consequence of the former. Shivering and coldness are symptomatic of the formation of spasm, and the dryness of the skin a consequence of spasm; so that although all the symptoms are imputable to the diathesis, yet the shivering and coldness and dryness of skin, appear to stand in the relation of cause and effect.

In paragraph CLIV, he says *the dryness of the skin admits the same explanation as the shivering and sense of cold.*

From what data does our author infer a higher degree of excitement in the brain and fibres of the muscles? The sense of languor that precedes disorders, originates from a diminished or increased energy of the whole system. The author boasts of the uniformity and simplicity of his principles. This increased energy of the brain does not square with his principles. If there exists in sthenic disorders a greater proportional excitement in the brain than any other part, why may not a less proportional energy of the brain and muscles exist in asthenic disorders? And what would this be but an admission of Cullen's theory? The author has taken great pains

to avoid the imputation of imitation, by adding the muscles to the brain. Cullen supposed the brain the origin of the nervous system, and the nerves distributed in the extreme vessels to be the principal seat of the operation of sedative and stimulant powers, and by a peculiar sympathy with the brain, any alteration in the state of the former, would be very readily communicated to that of the latter. But this reasoning does not apply to the system of Doctor Brown. What possible construction can be put upon the words "*conveniently borne by the excitability confined within certain boundaries.*" We are all sensible that neither the excitement nor excitability are infinite. They are confined within certain boundaries. We also know that the excitability may be raised or diminished. But why the excitability cannot bear the excitement raised without consequent suffering, the author has not informed us.

Paragraph CCCXXXVII. *The dryness of the skin is occasioned by the great excitement and density of the fibres that encircle the extreme vessels, diminishing their diameters to such a degree that the imperceptible vapour cannot be taken into them, or if taken in, cannot be transmitted.*

What fibres are those that encircle the extreme vessels? The muscles are longitudinal instead of circular. The skin cannot embrace the extreme vessels more closely in the morbid than healthy state, except a spasm is formed. Admit for a moment

moment that the extreme vessels were surrounded with circular elastic fibres, what proof can we advance that the excitement and their density is increased? Their density must be increased either as simple or living solids. If simple, there must be a greater cohesion of particles, and if so why not assign the causes of its production. If increased as living solids, it must be by a forcible and durable contraction, which is synonymous with spasm. But in a succeeding sentence—“*It is not spasm, it is not constriction from cold, but a sthenic diathesis somewhat greater upon the surface than any other part.*”

If the excitement is greater on the surface in sthenic disorders, why may it not be less in asthenic disorders than any other part? If the excitement is greater on the surface than any other part, it must originate from the greater sensibility of the surface, or a direct application of the exciting powers. As the state of increased excitement is the same with increased tone, and as increased tone is always accompanied with increased contraction, and contraction is synonymous with spasm, what is the specific difference between his theory and Doctor Cullen's.

In a following sentence he says, that the stimulant energy of heat succeeding cold, is a powerful exciting cause of sthenic diseases. The spring is the season most subject to inflammatory diseases. But I do not conceive the warmth of spring produces sthenic diathesis: for the effect

of any considerable degree of heat is to debilitate. The cold of winter braces and increases the tone of muscular fibres, and the irregular degrees of heat succeeding gives occasion to the appearance of inflammatory disorders. Their appearance is sometimes from partial application of heat. But in no instance can the heat be considered as the efficient cause. The heat is the occasional cause. Heat does not seem to produce diathesis, but to convert diathesis into disease.

Paragraph CCCLXI. What does the author mean by an inflammatory or catarrhal affection of some of the large joints accompanying a phrenitis? It is a novel and singular mode of speaking to apply the epithet *catarrhal* to affections of the joints. In a succeeding paragraph he says, an inflammation in its proper form does not exist in this disorder; but an approach to inflammatory state exists in the joints, in the muscles, and especially over the spine or about the chest, or in the bottom of the throat, or there is a catarrhal state.

From what source were these new symptoms and affections drawn. Not surely from the bedside of the sick. What is the proper form of inflammation? He admits that venesection gives great relief; and yet he says there is no inflammation, but a state particularly over the spine approaching to inflammation. From what does he infer that a state approaching to inflammation exists over the spine in preference to every other



part of the system. The same general remedies that are employed in other inflammations, are employed in this disease, and sometimes with the same salutary effect.

Paragraph CCCLXVIII. *Contagion ferments without any change of solids or fluids, it fills all the vessels and then is gradually ejected by the pores.*

Is it not unaccountable that a state of fermentation should exist without inducing any change in solids or fluids, contrary to its known effects in every substance which is subject to fermentation. What exempts the animal economy from this general law? The vital principle may modify the operation of any fermenting substance. But if it prevented any change taking place it must be by subduing the contagion in its first entrance into the body and the disorder would not exist. If no change is effected, what probable cause can be assigned for the most of eruptive disorders never occurring but once in the same person. This is a good ground for discriminating the exanthemata from the other phlegmasia.—They differ in their eruptive appearance, in their cause, although a similar method of cure answers for both in the same diathesis, I yet think it a good foundation of distinction. The other phlegmasiæ occur annually, and the exanthemata very seldom oftener than once in an age. The stimulant operation of contagion I doubt. 1st. Because eruptive disorders are often attended with manifest marks of debility and putrefaction

in their first appearance. This could not happen unless the force of stimulant operation was so great as to produce indirect debility. And this is improbable. 1st. Because contagious disorders generally appear in states of the atmosphere unfavorable to produce a sthenic effect. 2nd. That contagious disorders more commonly affect the weak and valetudinary than the strong and robust. Whereas, if their operation was indirectly debilitating, the strong and robust would be most subject to contagious disorders.

Contagions in general do not possess a stimulus sufficient to maintain the healthy state. Contagion is a matter foreign to the body, not necessary to support life, and from its evident effects on the body, we judge it to be often of a sedative nature. That catarrh uniformly succeeds heat as an efficient cause, as asserted in paragraph 411, I am very much disposed to doubt. It often succeeds heat I will allow, but in so far as it is a sthenic disease, I consider it as the offspring of cold. It is not always a sthenic disease, for it often occurs in weak debilitated habits. The alternation of heat with cold is apt to produce sthenic diathesis. But I am confident an equal, steady and uniform application of heat would never produce a catarrh. The affection of the fauces is a solid argument in support of an unequal degree of heat or cold having been applied. That it is not the sole effect of heat or cold is proven from its occurrence both in the spring and fall seasons. The good effect of sweating

in sthenic disorders I have no experience. From the testimony of others whose experience I have no reason to question, I believe it seldom contributes very considerably to their solution. Doctor Cullen says that sweating is seldom if ever serviceable in Peripneumony and as far as my experience goes, it exactly coincides with this opinion. I have observed critical sweats produce an entire solution of pneumonic inflammation. I have also observed that any considerable increase of perspiration was difficult to be raised, whether owing to a spasmodic state of the extreme vessels, or to an excessive excitement existing on the surface I will not decide. The means he employs to raise a sweat are of a stimulant nature. He advises laudanum and Dover's powders, together with a considerable covering of bed cloathes, means agreeably to his own principles, manifestly stimulant, and as such must serve to increase instead of diminishing the sum total of excitement. The balance between the different excretions of the system is not folly, the cure of many disorders depend upon their equilibrium. That such a balance exists the experiments of Sanctorious prove. But it is not so exact but occasional deviations may exist without impeding health.

No considerable diminution or increase can exist without disease being the inevitable consequence. It has long been a noted maxim that an increase of one evacuation is always followed by the diminution of another. Whenever a  
cure

cure of any disorder depends upon exciting any evacuation, the increase of another will certainly diminish that upon which health or a cure depends. Any considerable Catharsis in fevers is always accompanied with a dryness of the skin. A diarrhæa although an asthenic disorder, is often relieved by a determination of fluids towards the surface. If in inflammatory disorders a greater proportional excitement exists in the part affected than in other parts of the system, and if the fluids contained in the vessels stimulate; will, determining a greater quantity of fluids into them diminish their excitement. He acknowledges that there is an objection to sweating, because the means employed are generally of a heating stimulant nature. In a former page he asserted, that the application of cold was the most powerful means to diminish the excitement, and further that cold increased perspiration. From this concession the good effects of sweating is not very conspicuous on his own principles.

The cause of menstruation both in its first appearance, continuance and final cessation, our author imputes to a particular conformation of the uterine vessels and the stimulus of love. That a peculiar conformation of the uterine vessels different from all other red vessels exists I have no doubt. And that nature designed their evolution to be effected at a particular age. But that the stimulus of love contributes to the first appearance of menstruation, I deny. It may serve to keep up the the discharge after it is established. The passion of love does not exist antecedent to the discharge.

Love

Love seems to be the consequence of it. The genital system is almost independent of the general habit. It is true stimulating food, spiritous liquors, &c. excite the passion of love by an operation on the general system. The energy of excitement is not much greater in the general system at the time of menstruation than at any other period. The energy of the uterine vessels is no doubt enlarged. The author has thrown no new light upon the subject of menstruation. Barely imputing it to excitement, is a very miserable account of its production. The *why* and *wherefore* are not accurately ascertained. Does a partial or general plethora exist at the menstruating periods? What particular conformation of female œconomy gives occasion to the existence of plethora? The cause of love and menstruation are one and the same—they continue an equal length of time and then disappear. It becomes the author to ascertain upon mathematical principles why a handsome man will at that particular period excite the passion of love which the same person could not have excited a year before, and which the handsomest female never excites. A deficiency of menstruation may undoubtedly be the consequence of high sthenic diathesis in the uterine vessels as well as the general system. I can see no incompatibility in deficient menstruation originating from this state. It is no ways similar to any artificial discharge, therefore arguments drawn from them cannot apply to the menstrual discharge. The morbid states of this discharge are commonly to be ascribed to debility,

and



and I have no doubt a spasmodic state of the uterine vessels sometimes exists. The cause of an obstructed menstruation is often a want of enjoying the pleasures of love ; but its operation is on the mind weakening the whole system, and of consequence the energy of the uterine vessels. It is not a very late discovery that stimulants and nourishing diet remove the morbid states of this discharge. It has been a mode of cure in general use for many years. If he claims any merit in its introduction, it is an arrogant assumption. The scurvy has both by ancient and modern physicians, been imputed to salted and corrupted meat ; unfermented farinacea, want of exercise and almost every debilitating cause enumerated by Doctor Brown. Grief for the loss of liberty and friends, a dislike to their present employment, the awe which the severity of discipline keeps them in, are circumstances which have very little effect on the minds of sailors—they are commonly the most careless, easy and contented, of any description of men ; they generally evidence more uneasiness on land than when, at sea, and so far from having an aversion to their mode of life, they are uncommonly attached to it. The fear of battle has (if any) a very momentary influence with sailors. Doctor Brown is but little acquainted with the nautical character or he is determined to introduce a few new remote causes at the expence of truth. I cannot conceive what the author means by this disorder standing on a narrow foundation in former theories. It has never in any theory been ascribed to the single operation

operation of one cause, but to the aggregate operation of a variety. The proximate cause of scurvy in the theories of the present day, is said to be an acrimony or vitiation of the fluids. A correspondent debility of the solids is unquestionable. Some of the causes of scurvy seem to operate immediately on the solids and some on the fluids. A difference in aliment will undoubtedly alter the quality, mixture and consistence of the fluids. A person living altogether on animal food will have a difference in quantity and quality of fluids from a person living entirely on vegetable diet. The mass of blood made from animal food, possesses a greater stimulant power than blood made from vegetable diet. It is more quickly animalized, and runs more speedily into a state of putrefaction. From these considerations, I think the humoral pathology is retained with more propriety in this, than any other disorder. A further support of this opinion is, that the scurvy seldom appears, except in consequence of long continued use of salted provisions, and an entire deprivation of vegetable aliment. Vegetables used plentifully have a powerful effect in restoring the healthy state. I have no doubt that a return to land exercises, to a pure dry air, and to a healthy mode of living, will effect a cure. But whether it is an attachment to my early medical principles, or from reasoning, (for I have little experience in the scurvy) I am disposed to believe from the testimony of others whose veracity I have no reason to question, that the plentiful use of acids and acescent vegetables greatly expedites

expedites the cure. The motion of the vessel tends to induce a costive habit, and that circumstance may be supposed to have considerable influence in producing the disorder, and in retarding its cure. It is an opinion of mine, that heating cathartics are not so salutary as cooling aperient medicines. This opinion, if true, corroborates the notion of a dissolution of the fluids being the proximate cause of the disorder. Costiveness by hindering the discharge of the bile, pancreatic juice and other fluids into the intestines, occasions their absorption, and hence the degeneracy and acrimony of the mass of blood.

The gout is a disorder whose proximate cause is not well settled. Its appearances are so vague and anomalous, that a satisfactory pathology is difficult to establish. It is, however, confirmed by the experience of ages, that it is the legitimate offspring of luxury and intemperance. Ebriety and gluttony have uniformly been its antecedents. The notion of its being hereditary, our author says is a tale, or the fundamental part of his doctrine will fail. If he was willing to put the truth of his doctrine upon this issue, I would have no objection. But the superior utility of his principles may more handsomely be combatted on the grounds of merit. The faculty by their own experience may determine whether they have judged right and his doctrine fail, or they have been deceived and his doctrine be established. I will observe by the by, that I can conceive no incompatibility or incongruity be-

tween the gout's being an hereditary disease and the fundamental part of his doctrine. He acknowledges that a certain texture of the fibres or stamina is more favorable to the approach of certain diseases than the opposite form, and as the gout belongs to one form or the other, and as I expect he will grant that parents generally transmit to their offspring a constitution similar to their own, I can conceive no absurdity in denominating it an hereditary disease. The argument he employs to refute this opinion, I conceive not to be well adapted to this purpose. Does it prove the disorder not hereditary because Peter by adapting his excitement to his stamina may evade the gout of his father Paul? Surely not—For it can be as mathematically proven as the truth of his doctrine that descendants of Podagrics, cannot bear with impunity the effect of the same exciting powers as others. An equal excess of drinking or eating will have a greater tendency to fetch on a paroxysm of the gout in them, than in a person not possessing the same hereditary taint. It has been admitted by Doctor Cullen and others, that the son by temperance and exercise that is suiting his excitement to his stamina, may evade the gout of his father. Could the medical world be so long deceived. The medical world has been groping in a long night of darkness—and happy is it for the human race, that this great luminary has arisen to brighten the darkened hemisphere of medicine. I can observe nothing in the opinion fundamentally incompatible with genuine medical principles, and

as the weight and number of a testimony is in favor of its being hereditary, I shall believe it until the contrary is proven. The resemblance between the gout and dyspepsia is not very apparent. It is true that dyspepsia resembles the gout of the stomach ; but the distinguishing mark of the gout is its inflammatory appearance in the extremities. It is not a trifling immaterial circumstance—It is the criterion of the disorder. The gout has been connected with the gravel.

With respect to the cure of this afflicting complaint, I must observe that although evacuations would be extremely improper when the disorder has frequently recurred and marks of inanition exist, yet I conceive there may be instances in the beginning, in which venæsection and other evacuations would be admissible. A plethora may exist when there is great deficiency of vigor in the living solids. The first full meal, if properly digested and assimilated, will no doubt conduce to fill the arterial & venous system, and every succeeding indulgence in eating and drinking will serve to increale that effect until repeated indulgence carries the excitement up to 70°, at which point indirect debility takes place. And as this is an instantaneous change of condition in the system, the former fullness of the vessels must continue, without some evacuation is made. In such a condition of the system a small evacuation relieves from oppression ; the fibres contract, and the tone of the system is in some measure restored. It is even admissible in some cases of gangrene.



grene. A gradual reduction of high living will gradually produce the same effect.

Our author says, to prolong the intervals of health and prevent a recurrence of the paroxysms the remedies are all the reverse of the hurtful powers. The hurtful powers are full living, eating and drinking luxuriously. But the prescription is to take rich food plentifully, with the restriction of keeping within the stimulant range. If the disorder is not the effect of one full meal, as it most certainly is not, full living must be the hurtful power that induced the disorder, consequently should be refrained from. It will certainly take a larger quantity of food or exciting powers to raise the languid excitement when the system is oppressed with indirect debility, when the excitability is worn out, than before the approach of the disorder. Therefore, to keep your medicines within the stimulant range, you must increase the sum total of stimulant or hurtful powers. In a former part of this work we are directed in indirect debility, to desist from the use of stimuli gradually, to allow the wasted excitability to accumulate. Why a different prescription in the gout? Is it because our author cannot brook imitation? This would be to tread in the footsteps of his predecessors. Few have advised evacuations in this disorder. They have also bore testimony against a sudden transition from luxurious living to a very meagre scanty diet. Instead of recommending full living, plenty of animal food, and a sparing use of vegetable ali-

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ment, would it not have been more consistent with his theory and common sense, to have advised a more and more sparing use of animal food, exercise and a proper regulation of the non-naturals. This has been the gouty regimen from the earliest ages of physic, and constant experience has established its beneficial consequences. A thorough cure is not to be expected from medicines. All the diffusive stimuli on earth only serve to increase the disorder. Is it any wonder our author fell a victim to his own theory? Hard thinking to compose such a wonderful system of physic; hard drinking and luxurious eating must land every podagric in the world to come. A gradual reduction of the hurtful powers, is a rational consistent mode of cure. Accustoming the system to less and less of stimulus, the excitability will accumulate, the natural excitement will be restored, and the body be free from disorder. But a contrary plan will continue the gout, the opprobrium medicorum. What necessity for the distinction of the gout of stronger and weakened persons? Or why any necessity for constituting a mild and violent disorder? They differ only in the degree of excitement. They require the same medicines, and therefore the distinction is frivolous and unmeaning. This is not preserving unity and simplicity in theory.

Paragraph DCXCV. *In the cure of anarsaca we must invigorate the parts where the atony and laxity prevails, as in the skin.*

What

What evidence have we of a greater atony in the skin than any other part. The anasarca arises from atony of the exhalants that empty into the adipose membrane, and the absorbents which return the lymph into the mass of blood. This is surely an internal *local affection*, unless he means to confine the meaning of the phrase to abdominal or thoracic affections. The medicine he enjoins, stimulant food and peruvian bark, certainly have no peculiar or pre-eminent operation on the skin. He has in a former part of this work defied the world to find a medicine that possessed a specific power of operating on a single part. Medicines too are to be directed to the *excitement*, not to the *fluids or solids*, and the excitement is one uniform undivided property over all the system. This local indication comes a little aukward from Doctor Brown.

Paragraph DCXXXI. *Besides the general indication of cure for asthenia, that suited to this case (meaning the general dropsy) must be particularly directed to the whole vascular system, and especially about their terminations and the commencements of the absorbent veins.*

The remedies (he says) are the usual ones.—  
Do the usual stimulant remedies operate specifically on the terminations of the vascular system and the commencement of the absorbent veins? Certainly not. They invigorate the whole system, and if any one part labours under greater atony than another, we must raise the excitement

ment in the general system above the point of health, to raise the excitement of the local affection to the healthy state. Our attention in most cases should be directed to keep up an equality in the excretions, and a uniform exertion of the vital principle. The means to effect this end are very seldom your violent diffusible stimuli. But his medicines operate as it were by magic. We would conjecture that he possessed a knowledge of legerdemain, by the surprising instantaneous cures he performs with the diffusible stimuli. The application of heat can by no means be salutary, unless strong enough to excite a profuse perspiration, and even in that case the debilitating effects of heat would more than counterbalance the good effects of the discharge. Even frictions debilitate, unless the collection of water is small, or some evacuation made so as to give the fibres a power to contract. Every effort of muscular fibres to contract, unless successful, is followed with an encrease of debility. Hence results the propriety of ordering frictions of the legs in the morning only when the intumescence has subsided. The success of all stimulating means, must be the same, unless the complaint is incipient and very slight. An instance of anasarca with ascites, lately fell under my observation in which evacuant, and I believe tonic remedies, had been tried without any success. I advised Doctor Bond's pills *for the dropsy*—They were exhibited by the attending physician without any considerable discharge of water until punctures were made in the feet. The discharge  
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by the feet, although inconsiderable compared to the great quantity collected in the abdomen and adipose membrane, made such an alteration in the system, that by continuing the pills a fortnight, an entire evacuation of the water was effected. The evacuation by the feet relieved the distention. The muscular fibres contracted with facility. Muscular contraction is the mode in which medicines operate. And if the system is so distended as to prevent contraction, their operation will be similar to the operation of medicines in a cadaver.

Apoplexy and palsy are disorders of indirect debility. They may sometimes be accompanied with sthenic diathesis. They seldom if ever arise from directly debilitating powers. Extreme cold is said to produce a palsy of the members. Opium and many poisons produce an apoplexy, which has been denominated *apoplexia venenata*. Whether the operation of opium in producing this disorder, is directly or indirectly debilitating I will not determine. But I will observe that the operation of opium exhibits phenomena very much resembling the apoplexy. The pulse is slow, its energy diminished, a disposition to sleep, and a considerable want of sensibility. These symptoms appear so soon after the exhibition of opium, that we can hardly infer an indirectly debilitating operation. The good effects of opium in diminishing the deficiency of feeling on the surface, I doubt. Indeed opium possesses no specific power over the skin in preference to  
other

other parts. These are disorders of the nervous system, not cutaneous affections. I have known opium occasion an itching of the nose and skin; but it is such a rare occurrence that I impute it to idiosyncrasy. Evacuations are sometimes advisable on the same grounds: I have proven their propriety in the gout. But to procure as large evacuations as in peripneumony must be prejudicial. The same rule in the use of stimulants should be observed, as in the gout. They should gradually be left off. Let nature accommodate herself to a healthy quantity of stimulus.

From paragraph DCLVI, to DCLXVIII, the reader may see the author's theory of fevers.

That the phænomena of fevers in all their variety of types can be satisfactorily accounted for upon the principle of simple debility, is not a matter of doubt only, but positive falsehood. We will suppose that all the remote causes of fever, such as cold with moisture, heat with dryness, and scantiness of food, were to exert their united operation upon the sentient principle of life, and occasion a deficient excitement of the extreme terminations of the arteries, what would be the natural effect? A languor, lassitude and paleness would succeed, but no actual fever would be induced: It is impossible to account for the phænomena of fever, without having recourse to spasm and the *vis medicatrix*. The remote causes of fever occasion debility, but Doctor Cullen says it is not obvious how debility occasions



occasions spasm.\* He therefore solves the difficulty by imputing the spasm and cold stage to the operation of the *vis medicatrix*. The sudden transition from an apparent state of health to the cold stage of fevers, the stoppage of all the excretions, the shrinking of all the external parts, are indubitable proofs of a formation of spasm on the extreme arteries. Although, as Doctor Cullen says, it may be imputed to the weaker action of the heart and arteries, yet as these symptoms continue after the action of the heart and arteries seems to be restored, there is reason to believe a spasmodic constriction has taken place.† To give a mechanical and satisfactory account how the spasm is formed, would be a difficult, if not an impossible task. In this difficulty we are obliged to have recourse to the *vis medicatrix*. To raise a counteracting power, against the heart and arteries, would at first view appear to be a hurtful instead of a salutary power. But when we reflect that debility alone would never excite the action of the heart and arteries to restore the tone of the extreme vessels, the necessity of the spasm appears obvious. Spasm acts as an indirect stimulus to the sanguiferous system, thereby exciting the action of the heart and arteries to restore the tone. This will account for all the phenomena of fevers, and no other theory so satisfactorily. It appears to be a circuitous mode for nature to do her business in: but as the poet says, in human affairs—"a thousand

\* *Vide Cullen's First Lines, page 38.*

† *See Cullen's First Lines, page 36.*

stand movements scarce one purpose gain"—So in the operations of nature, it often takes two steps to gain one purpose. If debility was the only consideration to be contemplated as the proximate cause of fever, why do not the symptoms of fever appear in a more gradual manner? The first onset of fever is from an apparent state of health to coldness, shivering, pain in the head and bones, often delirium, and other symptoms of spasmodic constriction. Unless the author can prove debility to be instantaneous, it will by no means account for the phenomena of fevers. I have long thought there was something in fevers that had evaded the research of physicians, and every day's experience confirms me in that belief. Whether it is an unequal balance of the different parts of the system, or an unequal spasmodic constriction, or unequal debility, or the obstinacy of the two latter, I will not, cannot determine. But such is the fact, that after all the ingenious theories of fevers that have been published, still we often meet with cases that baffle the skill of the most experienced. I am convinced that if a fever can be reduced to simple, pure debility, the equality of the excretions restored, and the functions no way impaired but in vigor only, that we have it in our power by stimuli to restore the healthy state. It has for many years past been firmly believed both in this country and in Europe, that marsh effluvia produced intermittent and remittent fevers—and Doctor Cullen infers from the apparent debility in fevers, that they are of a sedative nature.

ture. Our author says he has proven that marsh effluvia are not the only cause of fevers. We admit it. But they are oftener the cause of fevers than he endeavors to make the public believe. He refers to paragraph DCLIII, IV, V, in which he treats of a tetanus, but I see no refutation of marsh effluvia being the remote cause of fevers. In consequence of this futile and imaginary refutation of marsh effluvia being the remote cause of fevers, he says that the change of excitement alone is the universal source of all general diseases. Would the author stifle inquiry and not advance a step in the investigation of causes. Marsh effluvia certainly exert a hurtful operation on the living system. Fevers occur most frequently in times and seasons most favorable to their generation. Dry seasons, when the waters become stagnant and the heat exhales a great quantity of vapour in the air, are the seasons of intermittent and remittent fevers. Other causes, as cold with moisture, a spare, scanty diet and excess in venery by their debilitating operation often contribute to produce the same effect. But as they are oftener and most generally the result of these noxious agents, they have been the peculiar objects of physicians' attention in tracing the remote cause of fevers. No one asserts that every different type of fever is to be referred or attributed to specifically different effluvia. The difference of type may be imputed to difference of constitution, to the different conditions of the same body, to the continuance of the disorder & debilitating powers; for we often see a  
quotidian

quotidian change to a tertian, and a tertian to a quartan. The difference of type may more properly be attributed to the greater or lesser operation of the effluvia, than to any specific change in its nature. It should appear that, if the cause of fever was debility, the less frequent the paroxysms the less debility, and e contra ; and it would seem from the same principle, that a less quantity of medicine would cure a quartan than quotidian. But this is not true. Some conjectures may perhaps obviate this difficulty.

1st. Although the debility in a quartan may not be as great as in a quotidian, the failure of the cortex and other medicines may be imputed to congestions of blood, or obstructions in the liver spleen or some of the viscera. The author may say that medicines altering the state of excitement would remove the local affection. But I am confident that stimulants often fail when the excitement is raised or lowered considerably, higher in one part than the general system, or in common medical language, when a local affection exists in a part.

2d. The spasm of the extreme vessels may not be so completely relaxed as in the paroxysms of other types. These or some other circumstances of the system frustrate the operation of medicines, by which it would appear, that a quartan is a disorder of greater debility than either of the other types. But I am sure the failure of medicine cannot be imputed to greater debility,

debility, for this reason, that a moderate quantity of the cortex will in oil commonly effect a cure of the quotidian and tertian types, when ten times the quantity will fail of a cure in the quartan. This I impute to some partial or general condition of the body not yet taken notice of or explained. A circumstance not yet observed is, that I have never seen the efficacy of the bark anywise considerable where the patient had a vigorous appetite. And as I have generally observed this to be the case in quartans (or very often so) we may infer that a certain condition of the stomach retards or impairs the operation of the cortex. Whether this state of the stomach consists in a redundant or scanty secretion of the gastric acid, or a greater proportional vigor in the muscular fibres of the stomach than any other part, I will not take upon me to determine.

The next absurdity of our author in his pathology of intermittent fevers, appears in his account of the succession of the three stages of fever in paragraph DCLXVI, in the following words.

*“Hence in a gentle degree of the disease, as cold is the most hurtful power, the consequence is, that its effect is gradually taken off by the agreeable heat of the bed or the sun, and the strength thereby gradually brought forth. The heart and arteries gradually excited by the same heat, acquire vigor, and at last excited in their perspiratory terminations by the same stimulus, the most hurtful symp-*

*tion being thereby removed, they restore the hot fit, and afterwards carry on the same process to the breaking out of sweat."*

To pass over the logic of the first sentence, and come to the merits of the subject—Can there be any thing more futile, absurd and groundless? Is it not known to every one who ever experienced a paroxysm of the intermitting fever, that neither the heat of the sun nor the greatest covering of clothes will abate the sensation of cold. Does he not know that a patient without any covering except his common apparel, may retire into a room where the direct rays of the sun never enter, and the hot fit will succeed the cold almost as soon (if not quite) as if he was loaded with clothes and exposed himself to the direct rays of the sun. It is the power contained in the animal œconomy of generating heat that brings on the hot stage, or in other words, the *vis medicatrix*.

Mr. Sterne, in his *Tristram Shandy*, says some people rise by hanging great weights to small wires: an observation applicable to this paragraph and the whole system of Doctor Brown. If heat was such a powerful mean in increasing excitement, how would people sustain the intense heat of forges, glass houses and many other mechanic employments in which fire is the principal agent? There is a certain degree of heat necessary to create agreeable sensations, but whenever it is considerably raised above that point it ceases to



be stimulant. Nothing except an insatiable unwarrantable desire to promulgate a new theory could have so blinded this author as to endeavor to palm such monstrous absurdities upon the public.

Doctor Cullen, in Aphorism XXXIV, says, as the hot stage of fevers is so constantly preceded by a cold stage, we presume that the latter is the cause of the former. And as the cold stage consists in a spasmodic constriction of the extreme vessels, thereby proving an indirect stimulus to the sanguiferous system, a reaction takes place, the spasm is overcome, the hot and sweating stages ensue and a complete solution of the paroxysm follows. I for my part have no doubt of this being the mode in which nature attempts to relieve herself of any hurtful powers raised against the body; as debility simply will not, does not excite the languid action of the heart and arteries; if this was not the case, what would hinder the debility to increase so in the perspiratory terminations of the arteries (as he fancies to call it) as at length to extinguish the vital principle in them entirely? The hurtful powers are constantly applied when the phænomena of the cold, hot and sweating stages appear, and if providence had not wisely implanted the *vis medicatrix* in our constitution, we should fall an easy prey to the noxious agents that surround us. I have an equal degree of infidelity in the exhibition of stimulants in the hot stage, as I have in his theory. It is customary in the hot stage of  
fevers

fevers to exhibit a dose of opium, but that it would be equally proper to exhibit a quantity of spirits possessing an equal degree of stimulant power (for we will admit the stimulant power of opium a moment) I deny. Opium is given in this stage on the foundation of its being an antispasmodic. Experience has proven it to have this effect. It promotes the secretions and thereby hastens the solution of the paroxysm, but no stimulant to my knowledge will so generally have this effect. Our author in a former part of his work has imputed to the operation of opium the power of increasing the excitement on the surface, which may be brought forward to explain its good effects in this disorder. Admit it had a peculiarly exciting effect on the surface, it would not prove that it generally increased tone and inflammatory diathesis.

The operation of opium is no ways analagous to that of other diaphoretics, and as no stimulant possesses a like power of promoting perspiration, I therefore conclude, that the operation of opium in this instance (and most if not all others) is *sui generis*. It will by no means prove the admissibility of other stimulants in the hot stage of fevers. In fevers of great debility where there are no considerable exacerbations; I admit we are to pay no regard to any stage, but to administer the most powerful stimuli. But when the paroxysms are clearly marked, I doubt and deny their admissibility. I have heretofore (I believe) asserted, that the cold and hot stages of intermittent fevers was an inflammatory condition

of the system, and I presume that stimulants would aggravate the paroxysm. An increase of fever, a dryness of the skin, would be the infallible consequence of the administration of powerful stimuli.

The power of the bark I can by no means esteem so slightly as Doctor Brown—to say that it will turn out next to an imposition, is imposition itself. It is a medicine of sovereign efficacy in all cases of debility and putrefaction. To impute its good effects to the vehicle conjoined with it, is a great degree of stupidity and wilful blindness. When given in milk or treacle, I have never been able to observe any material difference in its efficacy. I can see no greater analogy between spirits, opium and common food, than between the peruvian bark and food. Neither opium or spirits are ever converted into juices. I am sure of the two substances, opium and the cortex, that if either of them bears a greater analogy to the common supports of life than the other, that it is the latter. All barks possess a small quantity of farinaceous matter—neither spirits nor opium possess any of it. The Otahitei bread is the product of a tree; and I believe that any parts of vegetables would support life better than opium or any chemical product. Barks I believe contain a small quantity of acid, and I am not sure but a little saccharine matter. These remarks will be sufficient I trust, to refute his theory of fevers.

Paragraph DCLXXVIII. *The fluids by stagnating under the heat of the body, degenerate into*  
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that foreign quality in a more extensive sense, called corruption, but in a more uncertain one, putrefaction.

It is to be remarked that the heat of the mass of blood arises from friction and violent motion. From whence then is this superlative degree of heat in putrefaction? The debility of the vessels would prevent the generation of any extraordinary degree of heat. Indeed we would conclude a priori that a diminution of heat would be the certain consequence of the stagnation of fluids. Heat being a powerful stimulant, we would suppose it would counteract the debility of the solids. That the fluids even in a state of putrefaction, would contain a sort of *vis medicatrix*. Heat is one of the principal remedies of our author, in sthenic disease, and if the inherent heat, or the heat induced by the disorder be active on the established principles of this system, although it be the consequence of putrefaction, it would remove debility except its degree was so great as to induce indirect debility. If heat is a powerful agent in preserving the healthy state of excitement, it would be very easy to diminish occasional redundancies of that element. The stimulant power of heat would seldom exceed the exigencies of stimulant power in diseases of great debility. In a note on this paragraph, he says we are apt to refer every deviation of the fluids from their natural state to one of the three fermentations. I have never to my recollection, in any author, observed any state of the fluids com-  
pared

pared to the vinous or acetous fermentations. The changes that take place, in fluids circulating in living vessels are imperceptible, we are entirely ignorant of them, and therefore we can compare them to no changes in inanimate matter. The putrefaction in animals and inanimate matter both by smell and appearance, resemble one another very nearly.

The putrefaction of the circulating fluids is to be imputed to a diminution of vigor in the vessels whereby they are left to their own intestine commotion and are in a condition pretty similar to any other inanimate matter. But as long as the vessels retain their accustomed vigor, the changes (if any take place) are not discoverable, and I am confident that the inherent vigor of the vessels will always prevent either a vinous or acetous fermentation. The blood in every considerable deviation of the solids from their healthy tone and vigor, is followed with a correspondent change. The component parts may exist in different proportions, but their precise and definite qualities are not discoverable. Heat has been supposed the offspring of putrefaction. But is not putrefaction rather the offspring of heat? There is a degree of heat generated in putrefying substances, but it is not excited in the same manner, and is not equal in degree to the healthy temperature of the body. The heat (if any) in a state of putrefaction or mortification, is a morbid case of heat, and very seldom exists in the body. That the circulating blood

blood is a perfectly mild bland fluid, I will not assert, but to infer the contrary, because the urine, bile, and other secreted fluids are acrid, is a false illogical conclusion. The urine does not formally exist in the mass of blood, and its acrid taste and offensive smell are undoubtedly owing to a change taking place in the secretory organ. What are those diffusible stimuli whose operation it is often necessary to moderate. I have no doubt we may administer too large a quantity of stimulus. They then have a directly opposite effect to that intended. I have known stimulants and tonics act as cathartics. But in diseases of great debility we very often fail with the most powerful stimulants, when the excitability is not worn out neither by age nor the excessive use of stimuli. If disease consisted in debility only, and we possessed stimulants whose operation was necessary to be checked, that long list or catalogue of disorders which are now the *opprobrium medicorum* would no longer have an existence. A few sentences succeeding we have a formal account of the progressive manner in which excitement (when lost) is restored through all the intricate windings of the digestive organs until it reaches the perspiratory terminations of the arteries, the *primum mobile* of his system.

The communication of excitement like the electric fluid is instantaneous. The very general distribution of nerves, and the consent of the stomach with every other part, is such that any impression made on it is as readily communicated



to the most distant part as one situate more near it. This a mere parade of anatomical learning—It has no foundation in truth. The propriety of his five distinctions of local disorders I doubt—The second class in which he comprehends an inflammation of the stomach and intestines, appears devoid of foundation. I see no reason why an inflammation of the stomach and intestines has not as good a title to be denominated a general affection as an inflammation in the liver, spleen, tonsils, Trachea arteria, &c. The causes to which he imputes an inflammation of the stomach are more likely to exert their pernicious operation on the tonsils and œsophagus than on the stomach and intestines.

It is very seldom that ground glass gets into the stomach of any animal except rats. Cayenne pepper too is eaten by many people without the least injury, and in cases where fish-bones are swallowed, I never heard of an inflammation of the stomach being the consequence. The stomach has been known to dissolve the most solid bones, and as long as the stomach retains its digestive power an inflammation is not to be expected. Many instances of inflammation of the stomach occur where it can be traced to no mechanical injury, and as they are preceded by all the symptoms characteristic of general inflammation, we are justified in asserting these affections to be general. The author must explain the different organization of the stomach and intestines from the rest of the viscera before we concede to him  
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that they are not equally liable to general inflammation as the other viscera.

The fourth division of local disease is "*when contagion has been applied and generally diffused without affecting the excitement.*"

If as the author contends contagions possess a stimulant power, in what condition of the system do they not affect the excitement? It must be where a total torpor and inactivity of the system exists, or the contagion must be rendered inert, which is destroying the peculiar property of contagion. The venereal virus often occasions buboes and hernia humoralis, but these complaints when entirely local, do not come within the limits of his fourth division. For in a recent gonorrhœa the contagion is locally applied; we have no evidence of its general diffusion over the system. These complaints have been observed to attend a confirmed lues, and in this case they have no claim to local affections, for the virus affects the excitement often in a most serious and alarming manner. Swellings in the groins in pestilential fever or plague, although they are the sequel of a general disorder, cannot with propriety be said to be entirely local. Their cause being a contagious diathesis and a general affection will (as they depend on the diathesis) exclude them from local diseases. Independent of these objections to his division of local disorders, it would not be departing from the truth to affirm, that all disorders depend upon the diathesis — even

—even boils most commonly occur in plethoric sanguinary habits. It is the living principle which determines a greater quantity of blood to the part in boils and consequently inflammation —Further, we almost always employ general remedies in local disorders, and often with better effect than topical applications. Few complaints are of so trivial and unimportant a nature as not in some shape to affect the excitement.

The fifth part or division of local diseases, *“is where poisons enter the body and circulate through the system without effecting the excitement, except by producing some lesion or local injury, and in consequence of that injury disturbance over the system.”*

Vegetable poisons when applied to the surface of the body, often occasion fever & other symptoms of their operation on the excitement. Indeed it is impossible that poisons can be applied to so large a portion of the body as the skin, without affecting the principle of life or sensibility of the system. The mode of operation of vegetable poisons applied externally is not fully understood or explained. It may be by absorption of the effluvia that are emitted from vegetable poisons, or by a powerful stimulant operation. Whether contact is necessary to give effect to vegetable poison, I am uncertain; but I think that I have known persons affected without the recollection of any contact. In whatever manner they

they operate, I cannot conceive of a poison being generally diffused through the vessels without affecting the excitement. The cutaneous inflammation that originates from vegetable poisons, is (as Doctor Brown says of the universal cake of inflamed pustules in the small pox) such an exquisite stimulus, that the affection of the excitement must be the inevitable consequence. Even if the particles enter not the sanguiferous system, the disorder continues local but a very short time. And if they do not enter the system their effect, the disorder, does not come within the meaning or definition of this division of local disorders. The bites of poisonous animals cannot with any propriety be called a local disorder. The bite of an animal not poisonous is a local disorder. It is a mere solution of continuity and no otherwise affects the excitement than a solution of continuity made by an instrument, except the lips of the wound are generally more ragged, mangled and difficult to heal. But the poison of animals soon diffuses itself over the system, the excitement is raised to a violent height and death is often the unhappy consequence. It would puzzle the author or any of his adherents to advance many instances of poisons being generally diffused through all the vessels without producing an alteration of excitement. I do not recollect one solitary instance.

To conclude our remarks on this work, we will subjoin a few general observations.

Let us pause a moment—Let us concentré all  
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the rays of medical light that emanate from this system, into a focus. Can we say in sincerity that the author has any pretensions to originality of principle? Can we say, that the nicknamed Brunonian system is the system of nature? Can we say that he has ushered in the millineum of physic? The principle of excitement and excitability, are abstract, arbitrary principles. They are plainly derived from Cullen's theory of the mania. The idea was at least taken from Cullen's system, and although they are words which are as applicable as any language can afford to convey an idea of the state of the mind or body when we don't mean to point out their mechanical conditions, yet they convey no scientific idea of the state of the body in health and disease. The visionary hypothesis of Staahl, the supposed Centor of Boerhaave, the gas of Van Helmont, or the vital fire of Shebbeare, would either of them have suited his purpose as well. All the difference is, that the word excitement expresses the effect of the living principle, and former theorists have attempted to explain the nature, seat and modus operandi of the living principle. The term excitement is used by him as synonymous with the living state, and every one the least acquainted with language knows, that barely saying that the living state is in proper degree or otherwise, is saying nothing at all.—What practical utility is there in the boasted scale of the author. The vital principle (if a fluid) is of such a nature that a guaging rod cannot be immersed to point out its rise and fall. And although

though it may be affected by the density or tenuity of the circumambient air, the scale of our author can in no case point out the vicissitudes it undergoes. His system stands entirely detached and unconnected with anatomy, physiology and every branch of medicine. Does his language convey a philosophical idea of the states of health and disease?

Inquire what occasions the different secretions of the body, how muscular contraction is performed. Excitement is the answer. After this pertinent answer, what do we know more than before? Look through this boasted system. Are not the features of Cullen's system perceptible? Take notice of the excitement being higher on the surface than any other part. Take notice of medicine operating on the extreme vessels, and of the energy of the brain being diminished and restored. From what source are those ideas derived? In every other system, the author's have sought for the vital principle through the channel of anatomy. The knowledge of anatomy has always been considered important to lay the foundation of physiology. And as physiology is a branch of medicine that embraces the knowledge of the living principle, and explains the different condition of the simple solids both as connected with and detached from the living principle, an intimate connection exists between the two branches. But our author takes a shorter cut to science. He has invented a method to obtain a knowledge of language without



a previous acquaintance with the alphabet. Has the author improved nosology by discarding all former classification of disorders, and substituting his two forms, the *sthenia* and *asthenia*. Is he entitled to a very abundant score of merit for the coinage and application of these words to the science of medicine? Has he not multiplied instead of diminishing disorders by the introduction of his two forms and subdividing them into the mild and violent? Former nosologists may have made arbitrary arrangements; they may have made supernumerary distinctions. But, is the new nosology superior to the old? Is not the old nosology better calculated for the instruction of pupils than the new? I am always disposed to give every man that portion of merit to which he is entitled; but, after a thorough survey of the work before us, I cannot discover any thing new or useful, except the distinction of direct and indirect debility. And even this distinction has been mentioned by Dickinson, so that it is doubtful whether Doctor Brown was the author of it. Whoever first started the idea and the variety of practice founded on these distinctions, is entitled to some share of credit. The idea has been known and observed long before the publication of the *Elementa Medicinæ*. Doctor Rush, on the effects of spiritous liquors observes, that to break the habit of drunkenness, it is necessary to gradually diminish the quantity, or substitute some other stimulus. The idea was known and discovered before the invention of the term.

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The principles I have established or embibed will not allow me to go far in his mode of cure. But whenever I have attempted, I have uniformly failed. The success of an opposite plan of cure adopted by Doctor Rush in the yellow fever does not coincide and harmonize with his principles. The exhibition of Calomel and Jalap, cannot be justifiable on the principles of this system. Suppose the contagion induced indirect debility, the gradual diminution of stimuli would be the proper mode of cure. The variety of symptoms that occur in disorders, cannot be imputed to debility merely. I cannot assent to the identity and sameness of all the asthenic form of diseases. They differ not in degree of debility only. Their causes are often specifically different, their symptoms are as various and opposite as the symptoms of the other form. I will not deny that the mode of operation of all substances on the body resemble one another, and that they in some measure stimulate. But the question is whether their stimulant power is sufficient to maintain health? If they do not, they may with propriety be called sedative powers.

The extension of the stimulant plan of cure to so many disorders as the author has done, I do not think discovers a great acquaintance with the effect and operation of medicine. It was carried full far enough before the publication of the *Elementa Medicinæ*. Many disorders of apparent debility have given way to what he calls the debilitating plan, after the stimulant plan had been exhausted.

exhausted. The author, in my opinion fell a victim to his own theory. It has been productive of mischievous consequences in this country if I may rely on the judgment of others. Systems have in many instances had a baneful influence in the administration of medicine. This system has (I believe) seduced not only the young and inexperienced, but the old and wary. It is a false light. The author was not contented with barely delivering his opinion, but he must misrepresent the science of medicine prior to his discovery. I would ask any rational physician whether the sum total of medical power is enlarged by the discovery. Is not the system of Brown a corruption of Cullen's system? I pledge myself to the world, that had not Cullen's system made its appearance, that the system of Brown would never have seen the light. With all his art and labour to diversify it, the source from which it is drawn is obvious. The circumstances under which it was composed were suspicious. The difference between Cullen's and Boerhaave's system is, that the former discarded the humoral pathology and accounted for the most of disorders upon changes taking place in the solids. Is not Brown's system built on the same foundation? No. He directs his medicines to the excitement. But no change can be effected in the excitement, except the medicines alter the state of the solids. What idea does the author mean to convey by deficient excitement—excitement in proper degree or increased excitement? They convey superficial ideas of the state of the animal economy,

œconomy, and however fond the author may be of the novelty of expression, if they convey any other idea than debility or atony, and inflammatory diathesis of Doctor Cullen, I must own I cannot discover it.

As the basis of his pathology is the same as Cullen's, what merit can he claim, except the coinage of a few words of Greek derivation.— Instead of publishing a new system he has forcibly entered upon the premises of Cullen, endeavored to demolish the beautiful edifice the latter had reared, and like an ignorant architect, erected it in a new and bungling manner, whereby its beauty, simplicity, and utility are entirely destroyed. The style has no claim to elegance; in many places it is very deficient in perspicuity, and in no part of it is his sentences constructed in a neat, elegant manner. This would be a small fault, did it contain principles calculated and suitable to govern the administration of medicine.

We live in an enlightened age, and every principle should be brought to the test of experience. The system of Brown, would have appeared very well when men wrote systems in their closets. But in the present day, when experiment has become fashionable in the sciences, the truth of his principles will not be admitted except they agree with experience.

Fancy has made wild excursions, invention has been stretched in search of an unerring principle to govern the administration of medicine—but all  
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has been comparatively fruitless. Experience is the only infallible test of the operation of medicines. Theories may serve to guide and govern in general cases; but wherever a theoretical principle is held up as an unerring guide, we are sure to be deceived in its application. In no theory is it more conspicuous than the one under discussion; it attempts to make the application of his general principle too universal. Experience demonstrates its futility, and reason discovers that the principle is visionary and Utopian.

The term excitability can convey no other meaning than that the animal œconomy is capable of being excited and animated by certain external agents. And the term excitement, conveys no other meaning than that the body is under the operation of exciting powers. The philosophical mind will easily discern how far these terms go to explain the different states of health and disease. Had the author substituted heat, instead of excitement, it would have served him full as well to explain the pathology of disorders. For as the accumulation of excitability (if it is proper to ascribe it to the negation or absence of a positive principle) is nothing more or less than an absence of excitement, and as cold is nothing more or less than a privation of heat, I think the term heat and cold would have been full as apt to explain the healthy and morbid states of the animal œconomy as the terms used in the Elements of Medicine. Heat is a more familiar phrase than excitement, and more obvious to common capacity.

city. And as they both express the *effect* only of the living principle, the former appears to me preferable. What adds weight to this opinion is, that as heat in most cases appears to be in pretty exact proportion to the vigor of life, and cold on the other hand resembles a state of death—it appears most natural to give these terms place to the new coined phrases of Doctor Brown.

The scale of our author is inconsistent with itself. For agreeably to the true principles of this system whenever indirect debility commences, the excitement must fall, and as the excitement falls it must repass through the grade of health instead of rising to  $80^{\circ}$  the point of death.

The co-existence of indirect debility and increased excitement are incompatible. The excitement never can rise up to  $80^{\circ}$ . For the human body is capable of a certain degree of activity of muscular fibre, and as death never takes place until all exertion and activity are nearly or quite exhausted, it therefore clearly follows, that previous to death the excitement must be diminished in room of increasing to  $80^{\circ}$  his imaginary point of death. The condition of muscular fibre when the excitement has risen to  $70^{\circ}$  or fallen to  $10^{\circ}$  I acknowledge to be entirely different, and require a different treatment to restore the healthy state. But debility direct or indirect always implies want of excitement. Our author has arranged the gout in his scale as a disorder pro-

R ceeding



feeding from directly debilitating powers, and in  
 the paragraphs of which he treats of its nature  
 and mode of cure, he affirms it to be the offspring  
 of indirectly debilitating powers, and prescribes a  
 mode of cure adapted to direct debility. An in-  
 consistence of such consequence ought not to pass  
 unnoticed. If we may credit the reports of o-  
 thers, the gout often makes its appearance in per-  
 sons who live luxuriously without the addition of  
 directly debilitating powers. Persons of this de-  
 scription are not apt to abate their mode of living,  
 either in quantity or quality. On the other hand  
 it is asserted by men of experience, that the ap-  
 petite of podagrics are the most vigorous the day  
 preceding the attack. And it is often with the  
 utmost difficulty that physicians can prevail upon  
 podagric patients to observe that degree of ab-  
 stinence necessary to restore them to health. An  
 attempt to discriminate disorders as they originate  
 from direct or indirect debility, is equally, if not  
 more frivolous than nosological arrangements.  
 The same disorder may proceed from want or  
 an excess of exciting power. I acknowledge,  
 could we certainly discover the quantity of op-  
 eration of both sets of powers, it might serve  
 some purpose in the administration of medicine.  
 But they are often so combined, that it would be  
 with the utmost difficulty to ascertain the amount  
 and difference of their operation. This is evi-  
 dent in the author's arrangement of the gout ;  
 for he differs from himself and all former physi-  
 cians in ascribing the gout to low living, and in  
 affirming that although indirectly debilitating  
 powers

powers may have been applied a paroxysm of the gout never comes on without the application of directly debilitating powers.—And I will further observe, that the phrase *directly debilitating powers* is synonymous with sedative powers. It manifests a great degree of infatuation for a person to combat the chimeras of his own imagination.

The method of cure enjoined in this work appears to me a continual warfare with experience. The disorders of the climate of America, Scotland and the north of Europe, often partake of an inflammatory nature. They very seldom give way without premising some considerable evacuations prior to the administration of stimulants. The necessity of evacuations has been denied by many physicians, and particularly by Doctor Saunders, in his pamphlet on the efficacy of the cortex. But I have in many instances administered the cortex and wine without any considerable relief until evacuations were made by vomit, stool and perspiration. In remittent fevers of this climate, when they had baffled the power of stimulants, and when great marks of debility existed, I have seen manifest advantage by the administration of Calomel so as to produce two or three stools. In an obstruction of the Catamenia of a long standing, when paleness, want of appetite and a feeble pulse, all manifested great debility, I have observed considerable advantages from Cathartics and the application of Epispastics prior to the administration of tonics and stimulants.

lants. The good effects of the Epispastics I imputed to the evacuation. It not only the disorders of Northern climates, but the most putrid disorders of tropical regions are cured by evacuations, what shall we think of the boasted efficacy of stimulants. Venesection has lately been pretty generally adopted in the West-Indies in the yellow fever, and as we are informed with considerable success.

The inexperience of youth and the dogmatism of old age, have greatly impeded the improvement of the healing art. The character of Doctor Brown, seems to be a combination of both. When we depart from the field of experience and adventure into the regions of conjecture, we are liable to deception from every quarter. Pleased with the fancies of our creation, we hug them with unspeakable delight. They bar the mind against the reception of new ideas; and the mind designed for increasing perfectibility continues stationary.

The theory of Doctor Brown, can certainly in point of utility claim no pre-eminence over that of Doctor Cullen. And although the latter is beset with many difficulties, the former is open to more solid objections. A still further objection to the principles laid down by Doctor Brown, bolts in upon my mind. People who take a degree of exercise, sleep and food, sufficient to keep the excitement at 40°, the healthy state do still, from the operation of marsh effluvia and contagious

contagious matter fall into fevers. Could this happen if they were of a stimulating nature. A circumstance which renders it improbable is, that children and adults of weakly constitutions are more liable to fevers than persons of a robust and vigorous habit. If they induced fevers by an indirectly debilitating operation, the robust and the vigorous would be the most subject to these affections—For when the excitement stands at  $40^{\circ}$  or higher, a less degree of stimulant power would produce a disorder than when it stood at  $30^{\circ}$  or lower. The highly excited condition of the body, and the highly exciting power of contagion would soon throw the system into disorder. But experience evinces that the vigorous and healthy escape the effect of these noxious agents oftener than the weak and valetudinary. As all substances in nature agreeably to this theory have a homogeneous operation on the animal œconomy, provided we could concenter and apportion them to the excitability, why may not marsh effluvia and contagion nourish and support life in the same manner as vegetable and animal food. This, could it be reduced to practice, would be a most capital discovery and of the last importance to the sustenance of man. I presume however, that physicians have not so far given up their judgement as to have very sanguine expectations of realising this chimerical notion. If we appeal to an unerring guide, *experience*, we will be satisfied that in all conditions of the vital principle marsh effluvia and contagion exert a very baneful operation on the living system

tem, that under no modification are they suited to restore health, but on the contrary, peculiarly calculated to generate disease. Were they as our author asserts, of a stimulating nature, they would be equally as proper as any equal stimulant power in persons labouring under direct debility. A still further difficulty occurs in reconciling Doctor Brown's general principles with facts, is that the same quantity of ardent spirits taken on an empty stomach will induce ebriety much sooner than when the stomach is loaded with food. Whereas if intoxication was the effect of the indirectly debilitating operation of the liquor a full stomach as it must coincide by its stimulating power with the exciting or stimulating operation of ardent spirits would more certainly and sooner take place with an equal quantity of spirits on a full than an empty stomach. I believe it may be laid down as a pretty general rule, that all substances which offend the senses are of a sedative nature. We observe too, that aromatics as they possess an agreeable odor are of a stimulating nature. The former observation is strengthened by the well known fact that odors in some people induce *syncope*s. And although some odors that are pleasant to the most of people are offensive to others, yet as by an idiosyncrasy of constitution, they prove injurious, it by no means impairs the force of the observation that odors which are offensive to the senses are of a sedative nature.

A further proof of this observation is, that the  
senses

senses seem designed to discriminate between substances, which are deleterious, and those that are salutary. For we find that substances which offend both smell and taste generally give uneasiness when taken into the stomach. Animals too are dictated by the same criterion in the choice of food.

I have, in the course of these observations conceded a state of excitement above the healthy point to be synonymous with phlogistic diathesis. But I will remark a very striking difference in the treatment of a genuine phlogistic diathesis and a state of excitement supervening the use of diffusible stimulus in a disease of debility. In the former venæsection is always of manifest advantage, in the latter I conceive it would be very injurious. A forbearance in the use of stimuli in the latter, would be the most judicious procedure. A true phlogistic diathesis never can exist unless the digestive and assimilating organs have been vigorous, and a large quantity of nutriment extracted and applied to the fibres of the body. It is true that when any considerable injury has been inflicted in any part of the body, a phlogistic diathesis often occurs; but rarely (if ever) except the person enjoyed good health. The impression of stimuli on the stomach is transitory. And although by their frequent repetition we often invigorate the stomach to overcome the food, yet a complete restoration of health is not to be expected until a vigorous appetite and digestion has for some time existed.

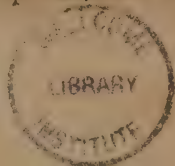
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The theory of Doctor Brown does not correspond with my experience. It is my opinion, that could the grave yards give an account why they are so populous, not a small share would be imputed to the new demonstrated system of medicine. The remarkable discovery that a certain degree of animation or excitement is necessary to the healthy state of every living substance, does not characterise this system the system of nature. The truth of that opinion no one will controvert, but the practical utility of the discovery is questionable. What light does it reflect on the growth and nourishment of vegetables to say that their healthy state depends on a certain arbitrary number of degrees of excitement. The toil and labour of our author reminds me of that passage of Horace—*Montes parturiunt nascetur ridiculus mus*.

The assertion of the author, respecting the sudden efficacy of stimulants in disorders which have heretofore been esteemed incurable, appear to me positive falsehoods. A system engendered through hatred and spite, and supported by improbable truths should not meet with a very favorable acceptance with the candid and judicious. I always suspect the truth of a man's assertion when he uses violence to impress belief. A simple relation of facts is sufficient for the modest and unassuming.

To conclude our remarks upon this work—I hope and trust that my brethren of the Faculty will not be seduced by this *ignis fatuus* into the wild, devious paths of experiment and medical enthusiasm.



# E R R A T A.

*Page. Line.*

- |    |   |         |                                                                     |
|----|---|---------|---------------------------------------------------------------------|
| 4  | — | 20      | for diversified, read diversified.                                  |
| 6  | — | 21      | for disorder—disorders.                                             |
| 13 | — | 19      | for was—be.                                                         |
| 15 | — | 11      | for resists—resist.                                                 |
| 19 | — | 1       | for operation—effect.                                               |
| 20 | — | 26      | for untagible—untangible.                                           |
| 22 | — | 14      | for deliterious—deleterious.                                        |
| 30 | — | 13      | omit the word <i>that</i> .                                         |
| 31 | — | 16      | inquiry—enquiry.                                                    |
| 33 | — | 21 & 26 | variolus—variolous.                                                 |
| 36 | — | 4       | dody—body.                                                          |
| 37 | — | 22      | descomposing—decomposing.                                           |
| 51 | — | 5       | overcome—overcame.                                                  |
| 53 | — | 20      | elementary—alimentary.                                              |
| 58 | — | 11      | centrated—concentrated.                                             |
| 60 | — | 24      | inquiry—enquiry.                                                    |
| 61 | — | 30      | for—instead of procrastinating—<br>read, rather than procrastinate. |
| 65 | — | 23      | omit the words, <i>that is</i> .                                    |
| 66 | — | 23      | increases—increase.                                                 |
| 68 | — | 22      | possesses—possess.                                                  |
| 70 | — | 9       | pleuretic—pleuritic.                                                |
| 84 | — | 23 & 27 | omit the word <i>other</i> .                                        |
| 90 | — | 24      | an—in.                                                              |











